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PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE,

THIRTY-FIRST MEETING,

HELD AT

MONTREAL, CANADA,

AUGUST, 1882.

SALEM:
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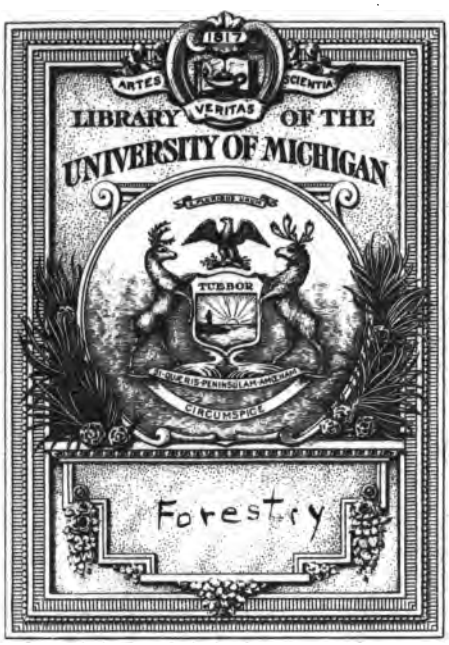
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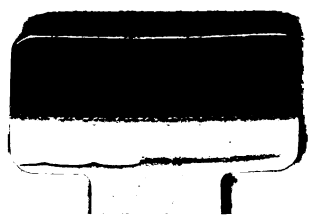

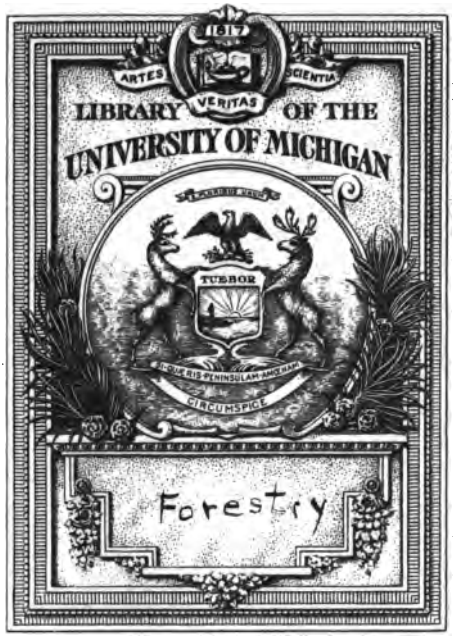

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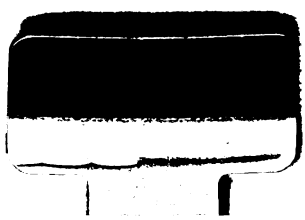

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1883.

EDITED BY

FREDERICK W. PUTNAM,

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Forestry

PRINTED AT

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John Torrance, M. H. Sanborn, R. S. C. Bagg, D. Morrice, J. T. Donald, G. W. Stephens, M. P. P., Geo. W. Craig, Hon. A. W. Ogilvie, G. B. Burland, Geo. Iles, Col. A. A. Stevenson, J. J. McLaren, James Shearer.

COMMITTEE ON ROOMS AND PLACES OF MEETING.

Chairman.—Dr. W. OSLER.
Secretary.—Prof. C. H. MCLEOD.

Dr. Harrington, Wm. Muir, Rev. Prof. Campbell, Frank Redpath, Prof. P. J. Darey, G. H. Chandler.

COMMITTEE ON PRINTING AND PUBLICATION.

Chairman.—S. E. DAWSON.
Secretary.—W. R. SALTER.

J. R. Dougall, Dr. L. H. Frechette, Richard White, Hugh Graham, C. A. Dansereau, H. Beaugrand, James Stewart, J. Tassé, M. P., F. Houde, M. P., L. O. David, J. C. Fleming, Rev. A. J. Bray, Col. A. A. Stevenson, J. J. McLaren, Q. C., R. W. Boodle, Dr. W. A. Molson, P. A. Peterson, F. B. Dakin, S. C. Stevenson, F. W. Hicks.

OFFICERS ELECTED

FOR THE

MINNEAPOLIS MEETING.

THIRD WEDNESDAY IN AUGUST, 1883.

PRESIDENT.

C. A. YOUNG of Princeton.

VICE PRESIDENTS.

A. Mathematics and Astronomy—W. A. ROGERS of Cambridge.
B. Physics—H. A. ROWLAND of Baltimore.
C. Chemistry—EDWARD W. MORLEY of Cleveland.
D. Mechanical Science—DE VOLSON WOOD of Hoboken.
E. Geology and Geography—C. H. HITCHCOCK of Hanover.
F. Biology—W. J. BEAL of Lansing.
G. Histology and Microscopy—J. D. COX of Cincinnati.
H. Anthropology—O. T. MASON of Washington.
I. Economic Science, and Statistics—F. B. HOUGH of Lowville.

PERMANENT SECRETARY.

F. W. PUTNAM, of Cambridge.

GENERAL SECRETARY.

J. R. EASTMAN of Washington.

ASSISTANT GENERAL SECRETARY.

ALFRED SPRINGER of Cincinnati.

SECRETARIES OF THE SECTIONS.

A. Mathematics and Astronomy—W. W. JOHNSON of Annapolis.
B. Physics—C. K. WEAD of Ann Arbor.
C. Chemistry—J. W. LANGLEY of Ann Arbor.
D. Mechanical Science—[Resigned.]
E. Geology and Geography—ALEXIS A. JULIEN of New York.
F. Biology—S. A. FORBES of Normal.
G. Histology and Microscopy—CARL SEILER of Philadelphia.
H. Anthropology—G. H. PERKINS of Burlington.
I. Economic Science, and Statistics—JOSEPH CUMMINGS of Evanston.*

TREASURER.

WILLIAM LILLY of Mauch Chunk.

MEMBERS OF THE STANDING COMMITTEE

FOR THE

MINNEAPOLIS MEETING.

Past Presidents.—JAMES D. DANA of New Haven; JAMES HALL of Albany; ISAAC LEA of Philadelphia; F. A. P. BARNARD of New York; J. S. NEWBERRY of New York; B. A. GOULD of Boston; T. STERRY HUNT of Montreal; ASA GRAY of Cambridge; J. LAWRENCE SMITH of Louisville; JOSEPH LOVERING of Cambridge; JOHN L. LECONTE of Philadelphia; J. E. HILGARD of Washington; SIMON NEWCOMB of Washington; O. C. MARSH of New Haven; GEORGE F. BARKER of Philadelphia; GEORGE J. BRUSH of New Haven; J. W. DAWSON of Montreal.

Vice Presidents of the last Meeting.—WM. HARKNESS of Washington; T. C. MENDENHALL of Columbus; H. C. BOLTON of Hartford; W. P. TROWBRIDGE of New Haven; E. T. COX of San Francisco; W. H. DALL of Washington; A. H. TUTTLE of Columbus; DANIEL WILSON of Toronto; ALEXANDER WINCHELL of Ann Arbor; E. B. ELLIOTT of Washington.

Officers of the Meeting.—C. A. YOUNG of Princeton; W. A. ROGERS of Cambridge; H. A. ROWLAND of Baltimore; EDWARD W. MORLEY of Cleveland; DE VOLSON WOOD of Hoboken; C. H. HITCHCOCK of Hanover; W. J. BEAL of Lansing; J. D. COX of Cincinnati; O. T. MASON of Washington; F. B. HOUGH of Lowville; F. W. PUTNAM of Cambridge; J. R. EASTMAN of Washington; ALFRED SPRINGER of Cincinnati; W. W. JOHNSON of Annapolis; C. K. WEAD of Ann Arbor; J. W. LANGLEY of Ann Arbor; ALEXIS A. JULIEN of New York; S. A. FORBES of Normal; CARL SEILER of Philadelphia; G. H. PERKINS of Burlington; JOSEPH CUMMINGS of Evanston; WILLIAM LILLY of Mauch Chunk.

From the Association at large.—One Fellow to be elected from each Section.

LOCAL COMMITTEE

FOR THE

MINNEAPOLIS MEETING.

CHAIRMAN.

Hon. GEORGE A. PILLSBURY.

SECRETARY.

Prof. N. H. WINCHELL.

TREASURER.

Hon. GEORGE A. PILLSBURY.

SUBCOMMITTEE ON INVITATIONS AND RECEPTION.

Chairman.—President W. W. FOLWELL.

SUBCOMMITTEE ON FINANCE.

Chairman.—J. C. SEELEY, Esq.

SUBCOMMITTEE ON TRANSPORTATION AND EXCURSIONS.

Chairman.—THOMAS LOWRY, Esq.

SUBCOMMITTEE ON ENTERTAINMENT, HOTELS, LODGINGS AND LUNCHEONS.

Chairman.—Hon. A. C. RAND.

SUBCOMMITTEE ON ROOMS AND PLACES OF MEETING.

Chairman.—Hon. EUGENE M. WILSON.

SUBCOMMITTEE ON PRINTING.

Chairman.— DAVID BLAKELY, Esq.

GENERAL COMMITTEE.

Hon. GEORGE A. BRACKETT; Hon. A. C. RAND; Hon. JOHN DE LAITTRE; Ex-Gov. JOHN S. PILLSBURY; W. W. MCNAIR, Esq.; Dr. WM. W. FOLWELL; CHARLES W. JOHNSON; Gen. A. B. NETTLETON; Hon. W. W. WASHBURN; T. B. WALKER; Hon. O. C. MERRIMAN; E. V. WHITE; Hon. H. T. WELLES; Col. H. G. HICKS; THOMAS LOWRY, Esq.; WINTHROP YOUNG; Hon. W. S. KING; DAVID BLAKELY; Hon. R. B. LANGDON; Hon. D. L. KIEHLE; J. C. SEELEY, Esq; ANTHONY KELLY; Dr. A. F. ELLIOTT; Hon. F. W. BROOKS; Hon. EUGENE M. WILSON.

SPECIAL COMMITTEES.

1. *Permanent Committee on Weights, Measures and Coinage.*

F. A. P. BARNARD, of New York,
B. A. GOULD, of Boston,
J. E. HILGARD, of Washington,
H. A. NEWTON, of New Haven,
J. LAWRENCE SMITH, of Louisville,
E. B. ELLIOTT, of Washington,
JOHN TROWBRIDGE, of Cambridge,
A. M. MAYER, of Hoboken.

2. *Committee to Memorialize the Legislature of New York for a New Survey of Niagara Falls.*

G. W. HOLLEY, of Niagara Falls,
J. T. GARDNER, of Albany.

3. *Committee on Membership.*

THE MEMBERS OF THE STANDING COMMITTEE.

4. *Committee on the best methods of Science Teaching in the Public Schools.*

E. L. YOUMANS, of New York,
A. R. GROTE, of New York,
J. S. NEWBERRY, of New York,
J. W. POWELL, of Washington,
N. S. SHALER, of Cambridge,
N. T. LUPTON, of Nashville.

5. *Committee on the Registration of Deaths, Births, and Marriages.*

E. B. ELLIOTT, of Washington,
J. B. KILLEBREW, of Nashville,
E. T. COX, of San Francisco,
F. B. HOUGH, of Lowville,
J. S. COPES, of New Orleans,
JOHN COLLETT, of Indianapolis.

6. *Committee on Standard Time.*

Or[illegible]ND STO[illegible], of Univ. of Va.
S. P. LANGLEY, of Allegheny,
E. C. PICKERING of Cambridge,
J. R. EASTM[illegible]N, of Washington,
LEWIS BOSS, of Albany,
LEONARD WALDO, of New Haven,
J. K. REES, of St. Louis,
G. W. HOUGH, of Lowville,
H. S. PRITCHETT, of Glasgow.

7. *Committee on Stellar Magnitudes.*

E. C. PICKERING, of Cambridge,
LEWIS BOSS, of Albany,
S. W. BURNHAM, of Chicago,
ASAPH HALL, of Washington,
WILLIAM HARKNESS, of Washington,
E. S. HOLDEN, of Madison,
SIMON NEWCOMB, of Washington,
C. H. F. PETERS, of Clinton,
ORMOND STONE, of Univ. of Va.
C. A. YOUNG, of Princeton.

8. *Committee on State Geological Surveys.*

G. C. SWALLOW, of Columbia,
JAMES HALL, of Albany,
JOHN R. PROCTER, of Frankfort,
N. H. WINCHELL, of Minneapolis,
W. C. KERR, of Raleigh,
J. W. POWELL, of Washington,
EDWARD ORTON, of Columbus.

9. *Committee to confer with Committees of Foreign Associations for the Advancement of Science with reference to an International Convention of Scientific Associations.*

T. STERRY HUNT, of Montreal,
ALEXANDER AGASSIZ, of Cambridge.
SIMON NEWCOMB, of Washington.

10. *Committee to confer with a Committee of the British Association for the Advancement of Science on the Annual Records of Science.*

J. W. DAWSON, of Montreal,
F. W. CLARK, of Washington,
EDWARD S. DANA, of New Haven,
H. W. MARTIN, of Baltimore,
C. A. WHITE, of Washington.

11. *Committee to devise a plan for the proper indexing of the Literature of the Chemical Elements.*

H. CARRINGTON BOLTON, of Hartford,
IRA REMSEN, of Baltimore,
F. W. CLARK, of Washington,
A. R. LEEDS, of Hoboken,
A. A. JULIEN, of New York.

12. *Committee on a Primary Meridian and an International Standard of Time.*

F. A. P. BARNARD, of New York,
C. A. YOUNG, of Princeton,
T. STERRY HUNT, of Montreal,
J. R. EASTMAN, of Washington,
CHARLES CARPMAEL, of Toronto,
E. B. ELLIOTT, of Washington.

13. *Committee on International Congress of Geologists.*

JAMES HALL, of Albany,
J. W. DAWSON, of Montreal,
J. S. NEWBERRY, of New York,
T. STERRY HUNT, of Montreal,
C. H. HITCHCOCK, of Hanover,
RAPHAEL PUMPELLY, of Owego,
J. P. LESLEY, of Philadelphia.

14. *Committee to confer with the United States Geologist in regard to coöperation between the Government and State Geological Surveys.*

JAMES HALL, of Albany,
T. STERRY HUNT, of Montreal,
J. S. NEWBERRY, of New York,
N. H. WINCHELL, of Minneapolis,
C. H. HITCHCOCK, of Hanover,
J. P. LESLEY, of Philadelphia,
G. H. COOK, of Brunswick,
E. A. SMITH, of Tuscaloosa,
JOSEPH LE CONTE, of San Francisco.

This Committee has power to add to its number.

15. *Auditors.*

HENRY WHEATLAND, of Salem,
THOMAS MEEHAN, of Germantown.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIST. SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Beck,*	B. Silliman, jr., C. B. Trego,	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,*	J. D. Whitney, M. B. Williams,	
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, jr.,		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	B. Silliman, jr., O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,	B. Silliman, jr., J. Lawrence Smith,		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,*	B. Silliman, jr.,		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†	Jeffries Wyman,*		B. Silliman, jr.

*Deceased.

†Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING.	DATE	PLACE.	PRESIDENT.	VICE-PRESIDENT.	GENERAL SECRETARY.	PERMANENT SEC'Y.	TREASURER.
1st	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,*		Walter R. Johnson,*		Jeffries Wyman.*
2d	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,*		E. N. Horsford, 1		A. L. Elwyn.
3d	Mar. 12, 1850,	Charleston, S. C.,	A. D. Bache,* 2		L. R. Gibbs, 3		St. J. Ravenel.* 4
4th	Aug. 19, 1850,	New Haven, Conn.,	A. D. Bache,*		E. C. Herrick,*		A. L. Elwyn.
5th	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,*		W. B. Rogers, 5	S. F. Baird,	S. F. Baird. 6
6th	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,*		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
7th	July 28, 1853,	Cleveland, Ohio,	Benjamin Pierce,*		J. S. St. John,* 7	S. F. Baird,	A. L. Elwyn.
8th	April 26 1854,	Washington, D.C.,	J. D. Dana,		J. Lawrence Smith,	Joseph Lovering,	J. L. LeConte. 8
9th	Aug. 15, 1855,	Providence, R. I.,	John Torrey,*		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th	Aug. 20, 1856,	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th	Aug. 12, 1857,	Montreal, Canada,	Alexis Caswell,* 9	Alexis Caswell,*	John LeConte,	Joseph Lovering,	A. L. Elwyn.
12th	April 28, 1858,	Baltimore, Md.,	Alexis Caswell,*10	John E. Holbrook,*†	W. M. Gillespie,* 11	Joseph Lovering,	A. L. Elwyn.
13th	Aug. 3, 1859,	Springfield, Mass.,	Stephen Alexander,*	Edward Hitchcock,*	William Chauvenet,*	Joseph Lovering,	A. L. Elwyn.
14th	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th	Aug. 15, 1866,	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,* 12	Elias Loomis, 13	Joseph Lovering,	A. L. Elwyn.†
16th	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.†
17th	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb, 14	Joseph Lovering,	A. L. Elwyn.†
18th	Aug. 18, 1869,	Salem, Mass.,	J. W. Foster,*	O. N. Rood,	O. C. Marsh,	F. W. Putnam, 15	A. L. Elwyn.†
19th	Aug. 17, 1870,	Troy, N. Y.,	T. S. Hunt, 16	T. S. Hunt,	F. W. Putnam, 17	Joseph Lovering,	A. L. Elwyn.†
20th	Aug. 16, 1871,	Indianapolis, Ind.,	Asa Gray,	G. F. Barker,	F. W. Putnam,	Joseph Lovering,	W. S. Vaux.
21st	Aug. 15, 1872,	Dubuque, Iowa,	J. Lawrence Smith,	Alex. Winchell,	E. S. Morse,	Joseph Lovering,	W. S. Vaux.
22d	Aug. 20, 1873,	Portland, Me.,	Joseph Lovering,	A. H. Worthen,†	C. A. White,	F. W. Putnam,	W. S. Vaux.
23d	Aug. 12, 1874,	Hartford, Conn.,	J. L. LeConte,	C. S. Lyman,	A. C. Hamlin,	F. W. Putnam.	W. S. Vaux.†

1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of A. L. Elwyn, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home 1st day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased. † Not present at the meeting.

MEETINGS AND OFFICERS OF THE ASSOCIATION (*Continued*).

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENT, SECTION A.	VICE PRESIDENT, SECTION B.	CHAIRMAN OF PERMANENT SUBSECTION OF CHEMISTRY.	CHAIRMAN OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	CHAIRMAN OF PERMANENT SUBSECTION OF MICROSCOPY.	CHAIRMAN OF PERMANENT SUBSECTION OF ENTOMOLOGY.
24th	Aug. 11, 1875.	Detroit, Mich.,	J. E. Hilgard,	H. A. Newton,	J. W. Dawson,	S. W. Johnson,	L. H. Morgan,*	———	———
25th	Aug. 23, 1876.	Buffalo, N. Y.,	W. B. Rogers,	C. A. Young,	E. S. Morse,	G. F. Barker,	L. H. Morgan,*	R. H. Ward,	———
26th	Aug. 29, 1877.	Nashville, Tenn.,	S. Newcomb,	R. H. Thurston,[1]	O. C. Marsh,	N. T. Lupton,	Daniel Wilson,†	R. H. Ward,	———
27th	Aug. 21, 1878.	St. Louis, Mo.,	O. C. Marsh,	R. H. Thurston,	Aug. R. Grote,	F. W. Clarke,	———[2]	R. H. Ward,[3]	———
28th	Aug. 27, 1879.	Saratoga, N. Y.,	G. F. Barker,	S. P. Langley,	J. W. Powell,	F. W. Clarke,[4]	Daniel Wilson,	E. W. Morley,	———
29th	Aug. 25, 1880.	Boston, Mass.,	L. H. Morgan,*	Asaph Hall,	Alex. Agassiz,	J. M. Ordway,	J. W. Powell,	S. A. Lattimore,	———
30th	Aug. 17, 1881.	Cincinnati, Ohio,	G. J. Brush,	Wm. Harkness,[7]	E. T. Cox,[8]	G. C. Caldwell,[9]	G. Mallery,	A. B. Hervey,	J. G. Morris.

PERMANENT SECRETARY.	GENERAL SECRETARY.	SECRETARY OF SECTION A.	SECRETARY OF SECTION B.	SECRETARY OF PERMANENT SUBSECTION OF CHEMISTRY.	SECRETARY OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	SECRETARY OF PERMANENT SUBSECTION OF MICROSCOPY.	SECRETARY OF PERMANENT SUBSECTION OF ENTOMOLOGY.	TREASURER.
F. W. Putnam,	S. H. Scudder,	S. P. Langley, T. C. Mendenhall,	E. S. Morse,	F. W. Clarke,	F. W. Putnam,	———	———	W. S. Vaux.
F. W. Putnam,	T. C. Mendenhall,	A. W. Wright,	Albert H. Tuttle,	H. C. Bolton,	O. T. Mason,	E. W. Morley,	———	W. S. Vaux.
F. W. Putnam,	Aug. R. Grote,	H. C. Bolton,	Wm. H. Dall,	P. Schweitzer,	———[2]	T. O. Summers, Jr.,	———	W. S. Vaux.†
F. W. Putnam,	H. C. Bolton,	F. E. Nipher,	George Little,	A. P. S. Stuart,	———[2]	G. J. Engelmann,	———	W. S. Vaux.†
F. W. Putnam,	H. C. Bolton,[5]	J. K. Rees,	W. H. Dall,[6]	W. R. Nichols,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.†
F. W. Putnam,	J. K. Rees,	H. B. Mason,	C. V. Riley,	C. E. Munroe,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.
F. W. Putnam,	C. V. Riley,	E. T. Tappan,[10]	Wm. Saunders,	A. Springer,[12]	J. G. Henderson,	W. H. Seaman,[11]	B. P. Mann,	W. S. Vaux.†

[1] In the absence of E. C. Pickering.
[2] The Subsection united with Sec. B.
[3] In the absence of G. S. Blackie.
[4] In the absence of Ira Remsen.
[5] In the absence of George Little.
[6] In the absence of A. C. Wetherby.
[7] In the absence of A. M. Mayer.
[8] In the absence of George Engelmann.
[9] In the absence of W. R. Nichols.
[10] In the absence of John Trowbridge.
[11] In the absence of S. P. Sharples.
[12] In place of H. W. Wiley, called away.
* Deceased.
† Not present

COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

TO INCORPORATE THE "AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

SECTION 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

SECTION 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

SECTION 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

SECTION 4. This act shall take effect upon its passage.

HOUSE OF REPRESENTATIVES, March 10, 1874.
Passed to be enacted,
JOHN E. SANFORD, *Speaker*

IN SENATE, March 17, 1874.
Passed to be enacted,
GEO. B. LORING, *President.*

March 19, 1874.
Approved,
W. B. WASHBURN.

SECRETARY'S DEPARTMENT,
Boston, April 3, 1874.
A true copy, Attest:
DAVID PULSIFER,
Deputy Secretary of the Commonwealth.

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.*

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons, and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, nomination by the Standing Committee, and election by a majority of the members and fellows present in General Session.

ART. 4. Fellows shall be nominated by the Standing Committee from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members and fellows present in General Session.†

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, not exceeding three for each section, may be elected; the nominations to be made by the Stand-

*As amended at the Cincinnati Meeting, August, 1881, and to take effect at the Montreal Meeting, August, 1882.

†When this article was adopted in 1874 the following clause was added, but was stricken out as inoperative when the constitution was amended in 1881:—

"But all persons who may be members at the time of the adoption of this constitution may become fellows by signifying their desire to this effect before the first day of August, 1875."

ing Committee and approved by ballot in the respective sections before election by ballot in General Session. Honorary Fellows shall be entitled to all the privileges of fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in General Session from the fellows, and shall consist of a President, a Vice President from each section, a Permanent Secretary, a General Secretary, an Assistant General Secretary, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Standing Committee. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the chairmen of their respective Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the Standing Committee shall determine. The Vice Presidents may appoint temporary chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of these sessions. He shall receive the records from the Secretaries of the

Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting. He shall receive proposals for membership and bring them before the Standing Committee.

ART. 13. The Assistant General Secretary shall be the Secretary of the Standing Committee. He shall give to the Secretary of each Section the titles of papers assigned to it by the Standing Committee.

ART. 14. The Permanent Secretary shall be the executive officer of the Association under the direction of the Standing Committee. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually a general report for publication in the annual volume of Proceedings. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least three months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Standing Committee, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Standing Committee, and shall pay over to the Treasurer such unexpended funds as the Standing Committee may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Standing Committee. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Standing Committee, and may employ one or more clerks at such compensation as may be agreed upon by the Standing Committee.

ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Standing Committee. He shall annually present to the Standing Committee an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Standing Committee, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Standing Committee.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

ART. 17. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Standing Committee as the President of the meeting. Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Assistant General Secretary, and Treasurer, shall be filled by nomination of the Standing Committee and election by ballot in General Session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the Section.

ART. 18. The Standing Committee shall consist of the past Presidents, and the Vice Presidents of the last meeting, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Assistant General Secretary, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Committee, provided there are at least five, shall form a quorum for the transaction of business. The Standing Committee shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Committee shall be held in the committee room at 9 o'clock, A. M., on each day of the meeting of the Association. Special meetings of the Committee may be called at any time by the President. The Standing Committee shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Committee. The Committee shall receive and assign papers to the respective sections; examine and, if necessary, exclude papers; decide which papers, discussions and other

proceedings shall be published, and have the general direction of the publications of the Association; manage the financial affairs of the Association; arrange the business and programmes for General Sessions; suggest subjects for discussion, investigation or reports; nominate members and fellows; and receive and act upon all invitations extended to the Association and report the same at a General Session of the Association.

ART. 19. The Nominating Committee shall consist of the Standing Committee, and one member or fellow elected by each of the Sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice President and Secretary of each Section shall be recommended to the Nominating Committee by a sub-committee consisting of the Vice President, Secretary, and three members or fellows elected by the Section.

MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Standing Committee may designate.

ART. 21. General Sessions shall be held at 10 o'clock, A. M., unless otherwise ordered, on every day of the meeting, Sunday excepted, and at such other times as may be appointed by the Standing Committee.

SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into nine Sections, namely:— A, *Mathematics and Astronomy;* B, *Physics;* C, *Chemistry, including its application to agriculture and the arts;* D, *Mechanical Science;* E, *Geology and Geography;* F, *Biology;* G, *Histology and Microscopy;* H, *Anthropology;* I, *Economic Science and Statistics.* The Standing Committee shall have power to consolidate any two or more Sections temporarily, and such consolidated Sections shall be presided over by the senior Vice President and Secretary of the Sections comprising it.

ART. 23. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary, shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. Meetings of the Sections shall not be held at the same time with a General Session.

ART. 24. The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

ART. 25. A paper shall not be read in any Section nor Subsection until it has been received from the Standing Committee and placed on the programme of the day by the Sectional Committee.

SECTIONAL COMMITTEES.

ART. 26. The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Standing Committee. No change shall be made in the programme for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself, must be returned to the Standing Committee with the reasons why it was refused.

ART. 27. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 28. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 29. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

ART. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

Printed Proceedings.

Art. 31. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers or abstracts ready for the press and forward them to the Permanent Secretary within this interval, otherwise only the abstracts or titles taken from the "title blanks" will appear in the printed volume. The Standing Committee shall have power to order the printing of any paper by abstract or title only. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Standing Committee. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Standing Committee. The Standing Committee shall also designate the institutions to which copies shall be distributed.

Local Committee.

Art. 32. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

Library of the Association.

Art. 33. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting. Not more than five books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Standing Committee.

Admission Fee and Assessments.

Art. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

Art. 35. The annual assessment for members and fellows shall be three dollars.

Art. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Standing Committee.

Art. 37. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

Accounts.

Art. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Standing Committee.

Alterations of the Constitution.

Art. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers entered and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Election of members.
7. Election of fellows.
8. Unenumerated business.
9. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22).
LILLY, GEN. WILLIAM, Mauch Chunk, Carbon Co., Pa. (28).

MEMBERS.[3]

Abbot, S. L., M. D., 90 Mt. Vernon St., Boston, Mass. (29).
Abbott, Dr. Chas. C., Trenton, N. J. (29).
Abbott, E. K., M. D., Salinas, Monterey Co., Cal. (29).
Abert, Col. James W., Newport, Ky. (31).
Abert, S. Thayer, 1907 Pennsylvania Avenue, Washington, D. C. (30).
Acker, E. O'C., Ass't Eng'r, U. S. N., Norristown, Pa. (28).
Adams, Frank, 41 McGill College Avenue, Montreal, Can. (29).
Adams, John S., M. D., Oakland, Cal. (29).
Adams, Julius W., 155 Congress St., Brooklyn, N. Y. (29).
Adams, Capt. Robert C., Montreal, Can. (31).
Agard, Dr. A. H., 1259 Alice St., Oakland, Alameda Co., Cal. (28).
Aikin, Prof. W. E. A., Baltimore, Md. (12).
Allen, Edmund T., 404 Market St., St. Louis, Mo. (27).
Allen, Dr. G. M., 102 Clark St., Cincinnati, Ohio (30).

[1] The numbers in parentheses indicate the meeting at which the member was elected. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. The payment of ten dollars at one time entitles a member to the subsequent volumes bound in cloth, or by the payment of twenty dollars, to the volumes bound in half morocco.

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications.

The names of Patrons are to remain permanently on the list.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The money derived from Life Memberships is invested as a fund, the income of which is to be used only to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

Allen, J. M., Hartford, Conn. (22).
Alling, J. C., Supt. City Schools, Albert Lea, Minn. (31).
Alling, Miss Mary R., Acra, Greene Co. N. Y. (29).
Allis, Solon M., Tucson, Arizona (29).
Ammen, Daniel, Rear Admiral U. S. Navy, Washington, D. C. (26).
Anderson, Rev. Joseph, Waterbury, Conn. (29).
Anderson, Newton M., Columbus, Ohio (30).
Andrews, L. W., Ph. D., Springfield, Mass. (31).
Angus, William, 214 St. James St., Montreal, Can. (31).
Appleton, Rev. Edw. W., D.D., Ashbourne P. O., Montgomery Co., Pa. (28).
Archambault, U. E., Montreal, Can. (31).
Armstrong, Lucius H., St. Nicholas, Duval Co., Fla. (30).
Armstrong, Mrs. Lucius H., St. Nicholas, Duval Co., Fla. (30).
Arnold, Lauren B., Rochester, N. Y. (30).
Arthur, J. C., Charles City, Iowa (21).
Ashburner, Charles A., 907 Walnut St., Philadelphia, Pa. (31).
Ashmead, Wm. H., Jacksonville, Fla. (29).
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26).
Atwater, Richard M., Millville, N. J. (29).
Atwood, E. S., East Orange, N. J. (29).
Atwood, Oscar, Rutland, Vt. (31).
Auchincloss, Wm. S., 209 Church St., Philadelphia, Pa. (29).
Aughey, Prof. Samuel, Univ. of Nebraska, Lincoln, Neb. (29).
Austin, Miss Mary, Mattapan Station, Milton, Mass. (31).
Ayres, Prof. Brown, Univ. of Louisiana, New Orleans, La. (31).
Ayres, Dr. S. C., 64 W. 7th St., Cincinnati, Ohio (30).

Babbit, John, Fredericton, N. B. (29).
Bailey, E. H. S., Bethlehem, Pa. (25).
Baker, A. L., 32 So. Charles St., Baltimore, Md. (30).
Baker, B. N., 32 So. Charles St., Baltimore, Md. (30).
Baker, Frank, M. D., 366 C St., Washington, D. C. (31).
Baker, Prof. I. O., Illinois Industrial Univ., Champaign, Ill. (30).
Baker, Prof. T. R., Millersville, Lancaster Co., Pa. (22).
Baker, Wm. E., 158 Tremont St., Boston, Mass. (29). [(31).
Baldwin, Miss Mary A., Summer Ave., Cor. Kearney St., Newark, N. J.
Baldwin, Very Rev. Dean Maurice S., Montreal, Can. (31).
Balen, Abraham D., Plainfield, N. J. (31).
Ballard, H. H., Lenox, Mass. (31).
Ballou, Nahum E., M. D., Sandwich, Ill. (29).
Ballou, Wm. H., 105 Dearborn Avenue, Chicago, Ill. (29).
Banes, Charles H., 2416 Spring Garden St., Philadelphia, Pa. (31).
Baptie, George, Normal School, Ottawa, Ontario, Can. (29).
Barclay, Robert, A. B., St. Louis, Mo. (30).
Barker, Martha M., care of H. R. Barker, Steam and Gas Fixtures Lowell, Mass. (31).

Barnard, Edward E., care R. Poole, Cherry and Union Sts., Nashville, Tenn. (26).
Barnard, Jas. M., Hotel Vendôme, Boston, Mass. (18).
Barnes, Charles J., Chicago, Ill. (27).
Barrows, Nathan, M. D., Hartford, Conn. (29).
Bartlett, Arthur K., Battle Creek, Mich. (29).
Bartlett, Prof. Edwin J., Dartmouth Coll., Hanover, N. H. (28).
Bartlett, Dr. Homer L., Flatbush, L. I. (28).
Bassett, Daniel A., Crawfordsville, Ind. (29).
Bassett, George W., M.D., Vandalia, Ill. (20).
Bassnett, Thomas, Box 936, Jacksonville, Fla. (8).
Bassnett, Mrs. Thomas, Box 936, Jacksonville, Fla. (24).
Bastian, Dr. David I., Clinton, Worcester Co., Mass. (25).
Bastin, Prof. E. S., Univ. of Chicago, Chicago, Ill. (29).
Batchelor, Charles, Electrician, Menlo Park, N. J. (28).
Batterson, J. G., Hartford, Conn.(23).
Bausch, Edward, Rochester, N. Y. (26).
Bayne, Prof. Herbert A., Royal Military College, Kingston, Ont. (29).
Beach, Charles M., Merchant, Hartford, Conn. (23).
Beach, J. Watson, Merchant, Hartford, Conn. (23).
Beach, William H., Beloit, Wis. (21).
Beadle, Delos W., St. Catharines, Ontario, Can. (31).
Beal, F. E. L., Agricultural College, Ames, Iowa (29).
Beall, Thomas, Lindsay, Ontario, Can. (31).
Beamer, Miles (30).
Bean, Thos. E., Box 441, Galena, Ill. (28).
Beaudry, J. Alphonse U., 97 St. James St., Montreal, Can. (31).
Beauregard, Gen. Gustave T., New Orleans, La. (30). [(29).
Beckwith, Henry C., Passed Ass't Eng'r U. S. N., Coleman's Station, N. Y.
Beebe, Wm. S., Brevet Major, U. S. Army, 25 Grace Court, Brooklyn, N. Y. (30).
Belknap, Chas., Lieut. U. S. N., U. S. S. Vandalia, care Navy Dept., Washington, D. C. (29).
Belknap, Morris B., Louisville, Ky. (29).
Belknap, Wm. B., Louisville, Ky. (29).
Bell, Alex. Melville, 88 Fayette St., Georgetown, D. C. (31).
Bell, John J., Exeter, N. H. (22).
Bell, Thomas D., McGill College, Montreal, Can. (31).
Bemis, Hon. A. S., President of the Common Council, Buffalo, N. Y.
Bemrose, Joseph, 158 P. O., Montreal, Can. (31). [(25).
Benjamin, E. B., 6 Barclay St., New York, N. Y. (19).
Benjamin, Marcus, 43 East 67th St., New York, N. Y. (27).
Berliner, Emile, 95 Milk St., Boston, Mass. (29).
Best, W. F., St. John, N. B. (29).
Beyer, H. G., Passed Ass't Surgeon, U. S. N., 125 Park Ave., Baltimore, Md. (31).
Bickmore, Mrs. Albert S., 12 East 41st St,, New York, N. Y. (31).

Bicknell, Hon. Thos. W., Boston, Mass. (29).
Bigelow, Otis, 605 7th St., Washington, D. C. (30).
Bigelow, Wm. Sturgis, M. D., 61 Beacon St., Boston, Mass. (29).
Bill, Charles, Springfield, Mass. (17).
Birge, Charles P., Keokuk, Iowa (29).
Blackford, Eugene G., 809 Bedford Avenue, Brooklyn, N. Y. (29).
Blackham, Dr. George E., Dunkirk, N. Y. (25).
Blackwell, Mrs. A. B., Somerville, N. J. (30).
Blaisdell, F. E., Poway, San Diego Co., Cal. (29).
Blake, Mrs. Clarence J., 19 St. James Avenue, Boston, Mass. (29).
Blake, Francis C., Penna. Lead Works, Mansfield Valley, Alleghany Co., Pa. (29).
Blake, Prof. John R., Davidson College, N. C. (29).
Blakslee, Prof. T. M., Harpersville, N. Y. (31).
Blanchard, Frederick, Box CC, Lowell, Mass. (29).
Blatchford, Eliphalet W., 375 North La Salle St., Chicago, Ill. (17).
Blatchford, John S., 13 Exchange St., Boston, Mass. (29).
Blodgett, A. N., M.D., Boylston St., Boston, Mass. (29).
Boardman, Miss E. D., 120 Beacon St., Boston, Mass. (29).
Boardman, Mrs. Wm. D., 38 Kenilworth St., Roxbury, Mass. (28).
Bobbitt, Mrs. John, Greensburg, Decatur Co., Ind. (30).
Boerner, Chas. G., Vevay, Ind. (29).
Bolles, Dr. Wm. P., 571 Dudley St., Dorchester, Mass. (29).
Bond, Geo. W., Boston, Mass. (29).
Bookwalter, J. W., Springfield, Ohio (30).
Bourland, Dr. Addison M., Van Buren, Ark. (29).
Bowditch, Frederic C., Box 615, Brookline, Mass. (29).
Bowers, Miss Virginia K., care Wm. Russell, Memphis Appeal Office, Memphis, Tenn. (27).
Bowker, W. H., 43 Chatham St., Boston, Mass. (30).
Bowles, George J., 1466 St. Catherine St., Montreal, Can. (31).
Bowles, Miss Margaretta, The Institute, Columbia, Tenn. (26).
Boyd, William, Cambridge, Mass. (29).
Boynton, Miss S. P., Box 150, Lynn, Mass. (19).
Brackett, John W., 581 Washington St., Boston, Mass. (29).
Brackett, Solomon H., St. Johnsbury, Vt. (29).
Bradford, Edward H., M. D., 150 Boylston St., Boston, Mass. (29).
Bradford, Royal B., Lt. Comd'r, U. S. N., Torpedo Station, Newport, R. I. (31).
Bradley, C. W., cor. 3d and Walnut Sts., Cincinnati, Ohio (27).
Bragg, Everett B., No. 3 Park Place, New York, N. Y. (29).
Brainerd, Thos. C., P. O. Box 720, Montreal, Can. (31).
Brandegee, T. S., Berlin, Conn. (29).
Branlette, Mrs. T. E., Louisville, Ky. (29).
Branstrup, Dr. W. T., Vincennes, Ind. (30).
Bray, Prof. C. D., College Hill, Mass. (29).
Bray, Mrs. Maria H., West Gloucester, Mass. (29).

Breckinridge, S. M., Box 2794, St. Louis, Mo. (27).
Brewster, William, 61 Sparks St., Cambridge, Mass. (29).
Brissette, Milton H., Montreal, Can. (31).
Bronnson, W. H., Lt. Comd'r U. S. N., U. S. C. and G. Survey, St'r "Blake," Navy Yard, New York, N. Y. (31).
Bronson, E. H., Ottawa, Ontario, Can. (31).
Brooks, Miss Margarette W., Salem, Mass. (29).
Broomall, Hon. John M., Media, Delaware Co., Pa. (23).
Brounell, Prof. Walter A., 725 Irving St., Syracuse, N. Y. (30).
Brown, Miss Anne, 528 West 7th St., Cincinnati, Ohio (31).
Brown, Arthur, C. E., Oakland, Alameda Co., Cal. (31).
Brown, Asa Warren, Newport, Ky. (30).
Brown, Mrs. Ashley, 115 Wilkinson St., Dayton, Ohio (25).
Brown, Prof. C. J., Randolph, N. Y. (31).
Brown, Fred. G., care of McKesson and Robbins, 93 Fulton St., New York, N. Y. (31).
Brown, Hon. John C., Vice President, Texas and Pacific R. R. Co., Pulaski, Tenn. (26).
Brown, Jonathan, 390 Broadway, Somerville, Mass. (29).
Brühl, Gustav, N. W. cor. John and Hopkins Sts., Cincinnati, Ohio (28).
Bryant, Wm. S., 61 Beacon St., Boston, Mass. (30).
Buck, C. Elton, Wilmington, Del. (29).
Buck, Henry C., West Somerville, Mass. (29).
Buckingham, Chas. L., U. S. Patent Office, Washington, D. C. (28).
Buffum, Miss Fannie A., Linden, Mass. (29).
Bull, John, Room 30, Corcoran Building, Washington, D. C. (31).
Bulloch, Walter H., 99 W. Monro St., Chicago, Ill. (30).
Burdick, Edson A., 408 Spruce St., Le Droit Park, Washington, D. C. (30).
Burke, Jos. C., Middletown, Conn. (29).
Burke, William, U. S. Patent Office, Washington, D. C. (28).
Burland, Jeffrey H., Montreal, Can. (31).
Burpee, J. P. C., St. John, N. B. (30).
Burr, Prof. William H., Troy, N. Y. (31).
Bush, Rev. Stephen, D. D., Waterford, N. Y. (19).
Butcher, Mrs. Z. D., Washington, D. C. (29).
Butler, A. W., Brookville, Franklin Co., Ind. (30).
Butler, Cyrus, 24 Cliff St., New York, N. Y. (29).
Bynner, Edwin L., 13 Pemberton Square, Boston, Mass. (29).
Byrnes, R. M., M. D., cor. 5th and Race Sts., Cincinnati, Ohio (30).

Cabot, Sam'l, jr., 53 Kilby St., Boston, Mass. (29).
Calder, Edwin E., Box 17, Providence, R. I. (29).
Caldwell, Albert G., Lt. Comd'r U. S. N., Torpedo Station, Newport, R. I. (31).
Caldwell, Dr. Frank, Cincinnati Hospital, Cincinnati, Ohio (30).
Calkins, Dr. Marshall, Springfield, Mass. (29).

Campbell, Rev. Prof. John, Presbyterian College, Montreal, Can. (31).
Canfield, Rev. Eli H., D.D., Arlington, Bennington Co., Vt. (25).
Capen, Miss Bessie T., Northampton, Mass. (23).
Cargill, H. L., St. Lawrence Hotel, Montreal, Can. (31).
Carman, Ezra A., Dept. of Agric., Washington, D. C. (30).
Carman, Lewis, Bangall, N. Y. (29).
Caron, C. K., Louisville, Ky. (30).
Carpenter, Geo. O., jr., care St. Louis Lead & Oil Co., St. Louis, Mo. (29).
Carpmael, Charles, Director of Magnetic Observatory, Toronto, Can. (31).
Carrington, Col. Henry B., U. S. A., care Adj. Gen. U. S. A., Washington, D.C. (20).
Case, Col. Theo. S., Ed. Western Review of Science, Kansas City, Mo. (27).
Cassino, S. E., Naturalists' Agency, 41 Arch St., Boston, Mass. (25).
Castle, Frederick A., M. D., 102 East 57th St., New York, N. Y. (29).
Castner, Dr. H. Y., 197 Pearl St., New York, N. Y. (30).
Caswell, John H., 11 West 48th Street, New York, N. Y. (26).
Chabert, L'Abbé Joseph, 80 St. Gabriel St., Montreal, Can. (31).
Chace, Arnold B., Valley Falls, R. I. (29).
Chadbourn, Erlon R., Lewiston, Me. (29).
Chadwick, James R., M. D., Clarendon St., Boston, Mass. (29).
Chance, H. Martyn, 2433 Fairmount Avenue, Philadelphia, Pa. (29).
Chandler, Prof. Chas. Henry, Ripon, Wis. (28).
Channing, Dr. Wm. F., Providence, R. I. (29).
Chapman, F. M., Danforth, Iroquois Co., Ill. (17).
Chapman, Henry C., M. D., 1226 Walnut St., Philadelphia, Pa. (29).
Charbonnier, Prof. L. H., University of Georgia, Athens, Ga. (26).
Chase, Prof. F. A., Fisk University, Nashville, Tenn. (26).
Chase, Mrs. Marine J., 1622 Locust St., Philadelphia, Pa. (31).
Chase, Prof. Pliny E., Haverford College, Haverford, Pa. (18).
Chase, R. Stuart, 16 Merrimack St., Haverhill, Mass. (18).
Chatfield, A. F., Albany, N. Y. (29).
Chenault, Prof. J. W., Louisville, Ky. (30).
Chester, Lt. Com'dr Colby M., U. S. N., Coast Survey Office, Washington, D. C. (28).
Childe, John Healey, Boston, Mass. (31).
Cisco, J. G., Jackson, Tenn. (29).
Clapp, Miss Cornelia M., Mt. Holyoke Seminary, South Hadley, Mass. (31).
Clark, John S., 64 Pinckney St., Boston, Mass. (31).
Clark, Simeon T., M.D., Lockport, Niagara Co., N. Y. (25).
Clarke, Miss Cora H., Jamaica Plain, Mass. (29).
Clarke, Prof. John M., Northampton, Mass. (31).
Clarke, Maurice D., 68 Ohio St., East Cambridge, Mass. (29).
Clarke, Robert, Cincinnati, Ohio (30).
Cleveland, J. L., 547 W. 8th St., Cincinnati, Ohio (30).
Clevenger, Dr. S. V., 189 37th St., Chicago, Ill. (29).
Cloud, Jno. W., Altoona, Pa. (28).
Coakley, George W., LL.D., Hempstead, L. I. (29).

Cobb, Samuel C., 235 Boylston St., Boston, Mass. (29).
Cobb, Rev. Sanford H., Saugerties, N. Y. (29).
Coffin, Amory, Phœnixville, Pa. (31).
Coffinberry, W. L., Grand Rapids, Mich. (20).
Colburn, Richard T., Elizabeth, N. J. (31).
Colby, C. E., M. E., School of Mines, Columbia College, New York, N. Y. (28).
Cole, Frederick, 1394 St. Catherine St., Montreal, Can. (31).
Colie, Edw. M., East Orange, N. J. (30).
Collier, M. Dwight, St. Louis, Mo. (27).
Colman, Henry, M.D., Swampscott, Mass. (25).
Colton, G. Woolworth, 182 William St., New York, N. Y. (22).
Comstock, Dr. T. Griswold, 507 North 14th St., St. Louis, Mo. (29).
Conger, Rev. Charles L., 1729 Eastern Avenue, Cincinnati, Ohio (30).
Conklin, W. A., Director Central Park Menagerie, New York, N. Y. (29).
Conro, Miss Emma Q., Keeseville, N. Y. (31).
Constable, Casimir, Constableville, N. Y. (29).
Cook, Dr. Charles D., 133 Pacific St., Brooklyn, N. Y. (25).
Coolidge, Herbert, 48 Kilby St., Boston, Mass. (29).
Coon, Henry C., M. D., Alfred Centre, N. Y. (29).
Cooper, Ed. M., 164 Walnut St., Cincinnati, Ohio (30).
Copes, Dr. Joseph S., New Orleans, La. (11).
Costin, Rev. M. P., St. Jno. College, Fordham, N. Y. (30).
Cotting, Dr. B. E., Roxbury, Mass. (31).
Coulson, Miss Mary, Oxford, Butler Co., Ohio (30).
Couzins, Miss Phœbe W., LL.B., 2647 Washington Ave., St. Louis, Mo. (27).
Covington, S. F., 68 W. 3d St., Cincinnati, Ohio (30).
Cowan, Richard Wm., 416 Notre Dame St., Montreal, Can. (31).
Cowdry, Thomas, M. D., Toronto, Can. (31).
Cox, Hon. Joseph, Madisonville, Ohio (30).
Cragin, Francis W., Washburn College, Topeka, Kan. (29).
Craig, Dr. Thomas (28).
Crandall, Prof. A. R., Lexington, Ky. (29).
Crane, C. R., 321 Garrard St., Covington, Ky. (30).
Crane, Henry L., 157 York St., Cincinnati, Ohio (30).
Crawford, John M., cor. 7th and Mound Sts., Cincinnati, Ohio (30).
Crawford, Prof. Morris B., Middletown, Conn. (30).
Cresson, Geo. V., 18th and Hamilton Sts., Philadelphia, Pa. (29).
Crevier, I. A., M. D., Montreal, Can. (31).
Crocker, Miss Lucretia, 40 Rutland Square, Boston, Mass. (29).
Crosby, Francis W., 20 Second St., N. E., Washington, D. C. (31).
Crosier, Dr. Edward S., Louisville, Ky. (29).
Crow, Wayman, St. Louis, Mo. (27).
Crowell, A. F., Woods Holl, Mass. (30).
Crump, Prof. M. H., Ogden College, Bowling Green, Ky. (29).
Cummings, John, Woburn, Mass. (18).

Currier, John McNab, M.D., Castleton, Vt. (28).
Curtis, Elizabeth, 16 Beethoven St., Egleston Square, Boston, Mass. (31).
Curtis, Dr. Lester, 1558 Wabash Avenue, Chicago, Ill. (29).
Curtiss, Abijah, Yonkers, N. Y. (31).
Cutter, John D., Box 276, New York, N. Y. (23).
Cutter, W. E., Box 1037, Worcester, Mass. (29).

Dale, Ben B., 25 Wiggins Block, Cincinnati, Ohio (30).
Dale, T. Nelson, jr., Gibbs Ave., Newport, R. I. (27).
Dansereau, C. A., Montreal, Can. (31).
Davenport, B. F., M. D., 751 Tremont St., Boston, Mass. (29).
Davenport, H. W., N. Y. and N. E. R. R., Norwood, Mass. (30).
Davis, Miss Anna M., 47 York St., Cincinnati, Ohio (30).
Davis, Miss Carrie, 47 York St., Cincinnati, Ohio (30).
Davis, Charles C., 58 W. 3d St., Cincinnati, Ohio (30).
Davis, G. Pierrepont, M. D., Hartford, Conn. (29).
Davis, J. J., M. D., Racine, Wis. (31).
Davis, Dr. John, 323 Elm St., Cincinnati, Ohio (30).
Davis, Dr. John L., 42 York St., Cincinnati, Ohio (30).
Davis, Leander S., Baker City, Baker Co., Oregon (29).
Davis, Dr. Wm. B., 310 Elm St., Cincinnati, Ohio (30).
Davis, Wm. J., Center and Walnut Sts., Louisville, Ky. (29).
Davison, E. W., Box 126, Baltimore, Md. (30).
Davison, Geo. W., Box 126, Baltimore, Md. (30).
Dawson, Miss Sarah M., 50 Sheridan Avenue, Roxbury, Mass. (29).
Day, Austin G., 120 Broadway, New York, N. Y. (29).
Day, Mrs. Oliver C., Kansas City, Mo. (29).
Dayton, L. M., 184 W. 2nd St., Cincinnati, Ohio (31).
Dean, Dr. D. V., St. Louis, Mo. (27).
De Beck, Dr. David, Cincinnati, Ohio (30).
DeCamp, William H., M. D., Grand Rapids, Mich. (21).
De Hass, Dr. Wills, Bureau of Ethnology, Washington, D. C. (30).
Delano, Joseph C., New Bedford, Mass. (5).
Dempsey, Peter C., Trenton, Ontario, Can. (31).
Denton, John M., London, Ontario, Can. (31).
Devereaux, Willard L., Clyde, Wayne Co., N. Y. (29).
Devereux, J. H., President C.C.C. & I.R.R., Cleveland, Ohio (18).
Dewey, Fred. P., Ph. B., Smithsonian Institution, Washington, D. C. (30).
Dexter, Julius, Cincinnati, Ohio (30).
Dickerman, Alton L., Ph. B., Somerville, Mass. (29).
Dickinson, Dwight, Surgeon U. S. N., Navy Dep't, Washington, D. C. (31).
Dickinson, Rev. John, 219 E. 27th St., New York, N. Y. (29).
Diller, J. Silas, Cambridge, Mass. (29).
Dimmick, L. N., M. D., Santa Barbara, Cal. (29).
Dinsmore, Prof. Thos. H., jr., York, Pa. (29).
Dixwell, Epes S., Cambridge, Mass. (1).

Dixwell, Geo. B., 553 Shawmut Avenue, Boston, Mass. (29).
Dixwell, John, M. D., 6 Pemberton Square, Boston, Mass. (29).
Dodd, Charles W., Mt. Auburn, Cincinnati, Ohio (30).
Dodd, Prof. James W., Vanderbilt Univ., Nashville, Tenn. (30).
Dodge, J. Richards, Washington, D. C. (31).
Dodge, Prof. James A., Minneapolis, Minn. (29).
Doggett, Mrs. Kate N., Chicago, Ill. (17).
Dolbear, Mrs. Alice J., College Hill, Mass. (29).
Donnelly, Ignatius, Hastings, Minn. (31).
Doolittle, Prof. C. L., South Bethlehem, Pa. (25).
Dorsey, Rev. James O., Bureau of Ethnology, Washington, D. C. (31).
Doughty, John W., Newburgh, N. Y. (19).
Douglass, Andrew E., 63 Pine St., New York, N. Y. (31).
Dow, Capt. John M., 69 Seventh Ave., New York, N. Y. (31).
Drowne, Prof. Charles, care William L. Drowne, Esq., Canaan Four Corners, N. Y. (6).
Drysdale, William, Montreal, Can. (31).
Dudley, P. H., 66½ Pine St., New York, N. Y. (29).
Dudley, Prof. Wm. R., Ithaca, N. Y. (29).
Duff, Mrs. W. L. (29).
Dummer, Edward, 19 Tremont Row, Boston, Mass. (31).
Dun, Dr. Walter A., Cincinnati, Ohio (31).
Dunbar, L. L., D. D. S., 23 Kearny St., San Francisco, Cal. (31).
Dunham, Carroll, jr., 5 Holyoke House, Cambridge, Mass. (31).
Dunham, Edw. K., 102 E. 57th St., New York, N. Y. (30).
Du Pré, Prof. Daniel A., Spartanburg, S. C. (28).
Durfee, Matthew C., A. B., care Bedford Stone Co., Cleveland, Ohio [(30).
Dury, Charles, 144½ Walnut St., Cincinnati, Ohio (30).
Dyche, Dr. D. T. D., Lebanon, Ohio (30).
Dyer, Clarence M., Lawrence, Mass. (22).

Earll, R. E., Waukegan, Ill. (28).
Eaton, Prof. James R., Liberty, Mo. (29).
Eaton, Hon. John, U. S. Commiss'r of Education, Washington, D. C. (25).
Eccles, Robert G., M. D., 290 Atlantic Ave., Brooklyn, N. Y. (31).
Eddy, C. W., Ware, Mass. (29).
Eddy, Fred. A. Bangor, Me. (29).
Edmunds, E. S., Lock Box 72, La Grange, Ind. (29).
Edwards, J. Baker, Montreal, Can. (31).
Edwards, J. M., Marlborough, Mass. (29).
Edwards, J. T., D. D., Randolph, N. Y. (31).
Edwards, Wm. H., Coalburgh, Kanawha Co., W. Va. (30).
Elliot, Dr. A. F., Minneapolis, Minn. (30).
Elliott, William, care Riggs & Co., 56 Wall St., New York, N. Y. (30).
Ellis, Wm. Hodgson, School of Practical Science, Toronto, Canada (25).
Elwyn, Alfred L., Philadelphia, Pa. (1).
Ely, Theo. N., Supt. Motive Power, Penn. R. R., Altoona, Pa. (29).

Emerson, Lowe, cor. John and Findlay Sts., Cincinnati, Ohio (30).
Emery, Albert H., Stamford, Conn. (29).
Emmerton, Mrs. W. H., Salem, Mass. (26).
Endemann, Hermann, Ph. D., 33 Nassau St., New York, N. Y. (26).
Enos, Alanson T., 28 Monroe Place, Brooklyn, N. Y. (27).
Estes, Dana, 301 Washington St., Boston, Mass. (29).
Evans, Dr. Arthur E., Cincinnati Hospital, Cincinnati, Ohio (30).
Evans, C. S., care Seth Evans, Clifton, Hamilton Co., Ohio (30).
Evans, Edwin, M. D., Streator, La Salle Co., Ill. (30).
Evans, Henry S., Montreal, Can. (31).
Everhart, J. F., M. D., Zanesville, Ohio (29).
Evers, Dr. Edw., 1737 North Market St., St. Louis, Mo. (28).

Fairchild, H. L., 153 E. 47th St., New York, N. Y. (28).
Fairfield, Frank H., 19 Broad St., Boston, Mass. (31).
Falconer, Wm., Botanic Garden, Cambridge, Mass. (29).
Fallon, John, Lawrence, Mass. (30).
Fanning, John T., Manchester, N. H. (29).
Farnam, Charles H., New Haven, Conn. (25).
Farnam, Prof. J. E., Georgetown College, Georgetown, Ky. (26).
Farnam, William W., New Haven, Conn. (28).
Farquhar, Edw., Asst. Lib'n, U. S. Patent Office, Washington, D. C. (29).
Farr, Henry L., Rutland, Vt. (31).
Farrar, Miss Elizabeth, 62 Lake Ave., Rochester, N. Y. (29).
Fearing, Clarence W., Mass. Inst. Technology, Boston, Mass. (29).
Feemster, J. H., 437 Broadway, Cincinnati, Ohio (30).
Felton, S. M., Box 2126, Philadelphia, Pa. (29).
Fenwick, A. G., M. D., London, Ontario, Can. (31).
Fernald, Prof. M. C., State Agricultural College, Orono, Me. (22).
Fernow, Bernhard E., Slatington, Pa. (31).
Ferrier, James, jr., 144 Metcalfe St., Montreal, Can. (31).
Ferrier, Walter F., 144 Metcalfe St., Montreal, Can. (31).
Fick, H. H., 78 Dayton St., Cincinnati, Ohio (30).
Fish, Chas., Brunswick, Me. (29).
Fisher, Chas. H., Sec. State Board of Health, Providence, R. I. (29).
Fisher, James H., Rochester, N. Y. (29).
Fitch, Dr. W. H., Rockford, Ill. (29).
Fitz, Prof. Newton, Norfolk, Va. (30).
Fleming, Sandford, Chancellor of Queen's Univ., Ottawa, Can. (31).
Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29).
Fletcher, Chas. R., 22 School St., Boston, Mass. (29).
Fletcher, James, Library of Parliament, Ottawa, Can. (31).
Fletcher, Lawrence B., Marlboro', N. Y. (29).
Fletcher, Dr. M. H., 65 W. 7th St., Cincinnati, Ohio (30).
Flint, Albert S., U. S. Naval Observatory, Washington, D. C. (30).
Flint, D. B., Watertown, Mass. (25).
Flint, Weston, Librarian U. S. Patent Office, Washington, D. C. (28).

Folger, Lt. Comd'r Wm. M., U. S. N., Care Bureau of Ordnance, Navy Dep't, Washington, D. C. (28).
Forbes, John, C. E., Dartmouth, N. S. (30).
Force, Cyrus G., jr., Cleveland, Ohio (31).
Forchheimer, Fred., M. D., 160 W. 9th St., Cincinnati, Ohio (30).
Forman, Benj. R., New Orleans, La. (30).
Fortescue, Mrs. Marion T., 341 W. 29th St., New York, N. Y. (31).
Fosgate, Dr. Blanchard, Auburn, N. Y. (7).
Foster, Henry, M.D., Clifton, N. Y. (17).
Foulkes, James F., M. D., Oakland, Alameda Co., Cal. (30).
Fox, George H., M. D., 18 East 31st St., New York, N. Y. (31).
Fox, Prof. Joseph G., Easton, Pa. (31).
Foye, Prof. J. C., Lawrence Univ., Appleton, Wis. (29).
Frazer, John, 80 Burnet Avenue, Cincinnati, Ohio (30).
Freeman, L. R., 274 W. 7th St., Cincinnati, Ohio (30).
Freeman, Spencer H., Johns Hopkins Univ., Baltimore, Md. (29).
French, Prof. Thomas, jr., Urbana, Ohio (30).
Frey, S. L., Palatine Bridge, Montgomery Co., N. Y. (29).
Frick, John H., Central Wesleyan College, Warrenton, Mo. (27).
Frisbie, J. F., M. D., Box 455, Newton, Mass. (29).
Froehling, Dr. Henry, Baltimore, Md. (29).
FROTHINGHAM, REV. FREDERICK, Milton, Mass. (11).
FROTHINGHAM, Mrs. Lois R., Milton, Mass. (31).
Fuchs, Julius, 411 W. Court St., Cincinnati, Ohio (30).
Fuller, Henry W., 17 Pemberton Square, Boston, Mass. (29).
Fulton, Prof. Robert B., University of Mississippi, Oxford, Miss. (21).
Fyles, Rev. Thomas W., Cowansville, P. Q., Can. (31).

Gaffield, Thomas, Boston, Mass. (29).
Gallaudet, Edw. M., Pres. Nat. Deaf Mute College, Washington, D.C. (28).
Gardiner, Edw. G., Boston Soc. Nat. History, Boston, Mass. (29).
Gardiner, Frederic, jr., Middletown, Conn. (29).
Gardner, Rev. Corliss B., Rochester, N. Y. (29).
Gardner, Joseph, M. D., Bedford, Lawrence Co., Ind. (30).
Garnett, Algernon S., M.D., Hot Springs, Arkansas (23).
Garrison, H. D., 3625 Vincennes Ave., Chicago, Ill. (31).
Garth, Charles, Montreal, Can. (31).
Gattinger, August, M.D., Nashville, Tenn. (26).
Gehring, J. Geo., 363 and 365 Pearl St., Cleveland, Ohio (29).
George, Henry, care D. Appleton & Co., New York, N. Y. (29).
George, James N., 40 Pearl St., Boston, Mass. (29).
Geppert, J. P., M. D., 305 Race St., Cincinnati, Ohio (30).
Gest, Mrs. J. J., 114 Dayton St., Cincinnati, Ohio (30).
Geyer, Wm. E., Stevens Inst. Technology, Hoboken, N. J. (29).
Ghequier, A. de, Box 316, Baltimore, Md. (30).
Giannelli, Chev'r A. M. F., Italian Consulate, Montreal, Can. (31).
Glauque, Florien, Cincinnati, Ohio (30).

Gilbert, Benj. D., Utica, N. Y. (31).
Girdwood, Gilbert P., 28 Beaver Hall Terrace, Montreal, Can. (31).
Gissler, Carl F., Ph.D., 63 Broadway, Brooklyn (E. D.), N. Y. (29).
Gladding, Thomas S., Box 1261, New York, N. Y. (30).
Glasgow, Wm., jr., 3016 Glasgow Place, St. Louis, Mo. (27).
Gleeson, Thos. W., 109 Court St., Boston, Mass. (31).
Glenny, William H., jr., Buffalo, N. Y. (25).
Glover, Miss Marie O., 321 Gates Avenue, Brooklyn, N. Y. (29).
Gold, Theodore S., West Cornwall, Conn. (4).
Goldsmith, Edw., 658 North 10th St., Philadelphia, Pa. (29).
Gordon, Prof. Jos. C., Deaf Mute College, Washington, D. C. (27).
Gordon, Dr. Thos. W., Georgetown, Ohio (30).
Gordon, W. J., Cleveland, Ohio (29).
Gore, James H., Columbian College, Washington, D. C. (29).
Gottschalck, Wilhelm von, M. D., 265 Benefit St., Providence, R. I. (29).
Gould, Sylvester C., Manchester, N. H. (22).
Graef, Edw. L., 40 Court St., Brooklyn, N. Y. (28).
Graf, Louis, Van Buren, Crawford Co., Ark. (30).
Grafflin, Col. C. L., Wilmington, N. C. (30).
Grant, Angus, Montreal, Can. (31).
Grant, Emory A., Louisville, Ky. (30).
Gratacap, L. P., Ph.B., M. A., American Museum Natural History, Central Park, New York, N. Y. (27).
Gray, Arthur F., Danversport, Mass. (29).
Graydon, Dr. T. W., 239 W. 4th St., Cincinnati, Ohio (30).
Green, Caleb, M.D., Homer, Cortland Co., N. Y. (28).
Green, Milbrey, M. D., 2389 Washington St., Boston, Mass. (29).
Greene, Jacob L., Pres. Mut. Life Ins. Co., Hartford, Conn. (23).
Greene, Thos. A., 146 Martin St., Milwaukee, Wis. (31).
Greenleaf, R. P., M. D., Henry Clay P. O., Del. (31).
Greenough, Malcolm S., 24 West St., Boston, Mass. (28).
Greenough, W. W., 24 West St., Boston, Mass. (29).
Greve, Theodor L. A., M. D., 260 W. 8th St., Cincinnati, Ohio (30).
Gridley, Albert L., Benzonia, Mich. (25).
Grinnell, George Bird, 39 Park Row, New York, N. Y. (25).
Grossklaus, John F., Navarre, Ohio (24).
Gulley, Prof. Frank A., Agricultural College, Oktibbeha Co., Miss. (30).
Gunning, William D. (22).

Habirshaw, Wm. M., 159 Front St., New York, N. Y. (29).
Hageman, John, 106 Bank St., Cincinnati, Ohio (29).
Hague, Arnold, 10 West 29th St., New York, N. Y. (26).
Haight, Stephen S., West Farms, New York, N. Y. (31).
Haines, Reuben, Wayne St., Germantown, Pa. (27).
Hale, Albert C., Ph. D., Box 129, Golden, Col. (29).
Hale, Miss Mary, care of Hon. J. J. Clarke, Kenilworth St., Roxbury, Mass. (31).

Haliburton, R. G., Q. C., Ottawa, Can. (31).
Hall, Prof. C. W., Univ. of Minnesota, Minneapolis, Minn. (28).
Hall, Prof. Lyman B., Haverford, Pa., (31).
Hallock, Albert P., Ph. B., 249 East 31st St. New York, N. Y. (31).
Halsted, Byron D., 245 Broadway, New York, N. Y. (29).
Hambach, Dr. G., 1419 Lami St., St. Louis, Mo. (26).
Hamel, Prof. Thomas E., Laval University, Quebec, Canada (18).
Hammond, Geo. W., Hotel Hamilton, Boston, Mass. (28).
Hammond, Mrs. Geo. W., Hotel Hamilton, Boston, Mass. (29).
HANAMAN, C. E., Troy, N. Y. (19).
Harding, Henry, Salem, Mass. (31).
Harding, Myron H., M. D., Lawrenceburg, Ind. (30).
Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28).
Hare, Prof. R. B., Hamilton, Ontario, Can. (28).
Harford, Wm. G. W., 2132 O'Farrell St., San Francisco, Cal. (29).
Harmon, Miss A. Maria, Ottawa, Can. (31).
Harper, Geo. W., Cincinnati, Ohio (30).
Harrington, W. H., Post Office Dept., Ottawa, Can. (29).
Harris, I. H., Waynesville, Warren Co., Ohio (30).
Harris, W. T., Concord, Mass. (27).
Harrison, Edwin, 322 Pine St., Room 20, St. Louis, Mo. (11).
Harrison, Geo. B., 520 Mulberry St., Bloomington, Ill. (29).
Harrison, Thos., LL. D., Fredericton, N. B. (29).
Hart, Charles P., M. D., Wyoming, Ohio (30).
Hart, Rev. Prof. Samuel, Trinity College, Hartford, Conn. (22).
Harvey, Charles W., Sup't Public Schools, Greensburg, Ind. (20).
Haste, Prof. R. A., Streator, Ill. (30).
Hatch, Horace, M. D., 1323 F St. N. W., Washington, D. C. (31).
Hatch, Dr. P. L., Minneapolis, Minn. (31).
Hatfield, Marcus P., M.D., Chicago Medical College, Chicago, Ill. (23).
Hathaway, Nath'l, School of Mines, Columbia College, New York, N. Y. (30).
Haven, Franklin, jr., New England Trust Co., Boston, Mass. (29).
Hawkins, Marion S., 60 High St., Nashville, Tenn. (26).
Hayden, Dr. F. V., 1910 Arch St., Philadelphia, Pa. (29).
Hayes, Francis B., Boston, Mass. (29).
Hayes, George E., 1125 Main Street, Buffalo, N. Y. (15).
Hayes, Richard, 700 Chestnut St., St. Louis, Mo. (27).
Haynes, Henry W., 239 Beacon St., Boston, Mass. (28).
Haywood, Prof. John, Otterbein Univ., Westerville, Ohio (30).
Heckman, Rev. Geo. C., Avondale, Cincinnati, Ohio (30).
Hedge, Fred. H., jr., Public Library, Lawrence, Mass. (28).
Hedges, Sidney M., 178 Devonshire St., Boston, Mass. (29).
Heighway, A. E., Cincinnati, Ohio (29).
Heighway, A. E., jr., 86 West 7th St., Cincinnati, Ohio (29).
Heighway, S. C., Cincinnati, Ohio (30).
Heine, Rev. Colborne, 168 Mance St., Montreal, Can, (31).
Hemiup, N. H., Minneapolis, Minn. (31).

Hempstead, G. S. B., M. D., Portsmouth, Ohio (29).
Henck, J. B., jr. (29).
Hendricks, Henry H., 512 Fifth Avenue, New York, N. Y. (30).
Hendricks, J. E., Des Moines, Iowa (29).
Henshaw, Samuel, Boston Soc. Nat. Hist., Boston, Mass. (29).
Hering, Prof. Dan'l W., Westminster, Md. (30).
Herrick, Clarence L., Minneapolis, Minn. (31).
Herrick, F. H., Burlington, Vt. (31).
HERRMAN, MRS. ESTHER, 59 West 56th St., New York, N. Y. (29).
Hersey, George D., M. D., 261 Broad St., Providence, R. I. (29).
Hertzberg, Prof. Constantine, 140 Duffield St., Brooklyn, N. Y. (29).
Hewitt, Charles N., Sec. State Bd. of Health, Red Wing, Minn. (31).
Heys, Thomas, Toronto, Can. (29).
Hibbard, Miss Sarah E., Montreal, Can. (31).
Hicks, John D., Old Westbury, Queen's Co., L. I. (23).
Hicks, John S., Roslyn, N. Y. (31).
Hicks, Prof. Lewis E., Denison Univ., Granville, Ohio (31).
Higginson, John A., 260 Clarendon St., Boston, Mass. (26).
Hilder, F. F., 1125 Bremen Avenue, St. Louis, Mo. (26).
Hildreth, Dr. John L., 37 Brattle St., Cambridge, Mass. (29).
Hill, Dr. H. H., cor. 5th and Race Sts., Cincinnati, Ohio (30).
Hill, Miss Lucy A., Lowell, Mass. (29).
Hills, Joseph L., 157 K St., South Boston, Mass. (31).
Hinckley, Miss Mary H., Mattapan Station, Milton, Mass. (31).
Hinds, Prof. J. I. D.; Cumberland Univ., Lebanon, Tenn. (26).
Hingston, Dr. William H., Montreal, Can. (31).
Hinton, John H., M. D., 41 West 32nd St., New York, N. Y. (29).
Hitchcock, Henry, 404 Market St., St. Louis, Mo. (27).
Hoadley, J. C., 29 High St,, cor. Federal, Boston, Mass. (29).
Hobbs, A. C., Bridgeport, Conn. (28).
Hodge, J. M., Greenup, Greenup Co., Ky. (29).
Hoeltge, Dr. A., 322 Lime St., Cincinnati, Ohio (30).
Hoffman, Walter J., M. D., Bureau of Ethnology, Smithsonian Institution, Washington, D. C. (30).
Hogg, Prof. Alexander, Fort Worth, Texas (26).
Holbrook, Alfred, Lebanon, Ohio (30).
Holbrook, F. N., E. M., 7 Jefferson St., Brooklyn, N. Y. (26).
Holbrook, Watson C., Coleta, Ill. (29).
Holbrooke, Prof. Geo. O., Trinity College, Hartford, Conn. (27).
Holland, John, 19 W. 4th St., Cincinnati, Ohio (30).
Holley, George W., Niagara Falls, N. Y. (19).
Hollick, Arthur, New Brighton, Staten Island, N. Y. (31).
Hollingsworth, Miss Rose, Mattapan Station, Milton, Mass. (31).
Holly, Jno. I., 62 Beaver St., New York, N. Y. (29).
Holman, Silas W., Mass. Inst. of Technology, Boston, Mass. (31).
Holmes, Edw. J., 60 Devonshire St., Boston, Mass. (29).
Holmes, Wm. H., Smithsonian Institution, Washington, D. C. (30).
Holstein, Geo. Wolf, Box 34, Belvidere, Warren Co., N. J. (28).

Holstein, L. M., Belvidere, Warren Co., N. J. (31).
Holt, Alfred F., M. D., Cambridge, Mass. (29).
Holt, Henry, 12 East 23d St., New York, N. Y. (29).
Homer, Chas. S., jr., of Valentine & Co., 323 Pearl St., New York, N. Y. (29).
Homes, Henry A., Librarian State Library, Albany, N. Y. (11).
Homes, Henry F., Albany, N. Y. (31).
Homsher, Dr. George W., Box 31, Fairfield, Franklin Co., Ind. (30).
Hood, E. Lyman, 296 Prospect St., Cleveland, Ohio (30).
Hood, Gilbert E., Lawrence, Mass. (29).
Hooper, Dr. F. H., 150 County St., New Bedford, Mass. (29).
Hopke, Theodore M., Hastings-on-Hudson, N. Y. (30).
Hopkirk, Prof. W. H., 505 Summer St., Burlington, Iowa (24).
Hoppock, A. E., Hastings-on-Hudson, N. Y. (29).
Horr, Asa, M.D., Dubuque, Iowa (21).
Horton, Rev. Sanford J., D.D., Cheshire, Conn. (28).
Hosea, Lewis M., JohnstonBuilding, Cincinnati, Ohio (30).
Hosmer, George M., 24 Church St., Boston, Mass. (29).
Hotchkiss, Jed., Staunton, Va. (31).
Hough, C. A., M. D., Waynesville, Warren Co., Ohio (30).
Houston, George M., Harrisonville, Mo. (26).
Hovey, Rev. H. C., New Haven, Conn. (29).
How, W. Storer, 129 W. 7th St., Cincinnati, Ohio (30).
Howard, Dr. Elmira Y., 170 W. 9th St., Cincinnati, Ohio (30).
Howe, Allen B., Ph. D., New Haven, Conn. (31).
Howe, Andrew J., M.D., Cincinnati, Ohio (28).
Howell, Edwin E., Rochester, N. Y. (25).
Howland, Edw. P., 211 4½ St., Washington, D. C. (29).
Hubbard, George W., M.D., Nashville, Tenn. (26).
Hudson, George H., Plattsburgh, Clinton Co., N. Y. (31).
Huling, Ray G., Fitchburg, Mass. (31).
Hulst, Rev. Geo. D., 15 Himrod St., Brooklyn, N. Y. (29).
Humphrey, D., M.D., Lawrence, Mass. (18).
Humphreys, A. W., Box 1384, New York, N. Y. (20).
Humphreys, Fred H., M. D., 109 Fulton St., New York, N. Y. (30).
Humphreys, Dr. Frederick, 109 Fulton St., New York, N. Y. (29).
Hungerford, Edward, Burlington, Vt. (31).
Hunt, A. M., Cadet Engr. U. S. N., Navy Dept., Washington, D. C. (29).
Hunt, Harry J., 41 E. 3d St., Cincinnati, Ohio (30).
Hunt, Jacob H., M. D., 41 E. 3d St., Cincinnati, Ohio (30).
Hunt, Miss Sarah E., Salem, Mass. (20).
Huntington, Prof. Chester, Clinton, Oneida Co., N. Y. (26).
Huntington, George H. (30).
Hurd, E. O., 4 W. 3d St., Cincinnati, Ohio (30).
Hutchinson, Charles W., Utica, N. Y. (31).
Hyatt, Jonathan D., Morrisania, N. Y. (29).
Hyatt, Col. Theodore, Pres. Penn. Military Acad., Chester, Pa. (30).
Hyde, Alfred T. (30).

Iddings, Joseph P., Orange, N. J. (31).
Iles, George, Windsor Hotel, Montreal, Can. (31).
Ives, Hubert R., 791 Sherbrooke St., Montreal, Can. (31).

Jack, John G., Chateauguay Basin, P. Q., Can. (31).
Jackson, Chas. C., 24 Congress St., Boston, Mass. (29).
Jackson, Frederick, Newton, Mass. (29).
Jacobs, Mrs. Sarah B., Bulfinch St., Boston, Mass. (29).
Jacques, Wm. W., 95 Milk St., Boston, Mass. (29).
James, Bushrod W., M. D., N. E. cor. 18th and Green Sts., Philadelphia, Pa. (29).
James, Davis L., 177 Race St., Cincinnati, Ohio (30).
James, Frederick H., M. D., Lancaster, N. Y. (25).
James, Henry, M. D., City Belleville, Ontario, Can. (29).
James, Mrs. Isabella, Brattle St., Cambridge, Mass. (31).
Janney, Reynold, Wilmington, Clinton Co., Ohio (30).
Jayne, Horace F., 1826 Chestnut St., Philadelphia, Pa. (29).
Jenkins, Prof. Oliver P. (30).
Jenney, Herbert, cor. 4th and Walnut Sts., Cincinnati, Ohio (30).
Jesup, Morris K., 52 William St., New York, N. Y. (29).
Jewell, Pliny, Mineralogical Amateur, Hartford, Conn. (23).
Jewett, Henry S., M. D., 21 S. Ludlow St., Dayton, Ohio (30).
Johnson, Arthur B., Utica, N. Y. (29).
Johnson, Prof. Hosmer A., Academy of Sciences, Chicago, Ill. (22).
Johnson, William S., 1221 Connecticut Ave., Washington, D. C. (31).
Johnston, Miss Elizabeth B., 937 K St., Washington, D. C. (30).
Johnston, Sanders W., Counsel at Law, 1427 F St., Washington, D. C. (29).
Jones, Miss Hattie L., care Supt. Cincinnati Hospital, Cincinnati, Ohio (30).
Jones, Henry A., Portland, Me. (29).
Jones, Rev. Hosea W., Delaware, Ohio (30).
Jones, Louis Q., 42 Farmington Ave., Hartford, Conn. (31).
Jones, Prof. Richard W., University of Mississippi, Oxford, Miss. (25).
Jones, Wm. A., Major of Engrs. U. S. A., Presidio of San Francisco Cal. (29).
Jordan, Prof. David S., Bloomington, Ind. (31).
Joseph, J. H., 16 Sacrament St., Montreal, Can. (31).
Judd, Hon. David W., 245 Broadway, New York, N. Y. (29).
Judge, Prof. J. F., cor. 6th and Mound Sts., Cincinnati, Ohio (30).
Julich, Wilhelm, 71 Bowery, New York, N. Y. (29).
Justus, Irving J., Stanfordville, Dutchess Co., N. Y. (28).

Kebler, Eliot A., care John Kebler, Cincinnati, Ohio (30).
Keen, Wm. W., M. D., 1729 Chestnut St., Philadelphia, Pa. (29).
Keenan, Mrs. M. V., 238½ Elm St., Cincinnati, Ohio (30).
Kellicott, David S., Buffalo, N. Y. (31).

Kellogg, David S., M. D., Plattsburgh, N. Y. (29).
Kellogg, James H., 1 Ida Terrace, Troy, N. Y. (29).
Kellogg, John H., M.D., Battle Creek, Mich. (24).
Kelsey, Prof. Ambrose P., Clinton, N. Y. (29).
Kemper, Dr. And. C., 101 Broadway, Cincinnati, Ohio (30).
Kennard, Mrs. Caroline A., Brookline, Mass. (29).
Kennedy, Prof. George T., Wolfville, N. S. (29).
Kennedy, John, 113 University St., Montreal, Can. (31).
Kerr, Wm. H., Mass. Inst. of Technology, Boston, Mass. (29).
Kershner, Prof. Jefferson E., Lancaster City, Pa. (29).
Kersting, Dr. H., East Eagle St., East Boston, Mass. (29).
Keyt, A. T., M. D., Walnut Hills, Cincinnati, Ohio (30).
Kidder, H. P., 40 State St., Boston, Mass. (29).
Killebrew, J. B., Commissioner of Agric., Nashville, Tenn. (26).
Kimball, John Cone, Commonwealth Hotel, Boston, Mass. (30).
Kinder, Miss Sarah, 27 Lockerbie St., Indianapolis, Ind. (20).
King, A. F. A., M. D., 726 13th St. N. W., Washington, D. C. (29).
King, Miss Harriet, Salem, Mass. (28).
King, Hiram U., Stamford, Conn. (31).
King, Mrs. Mary B. A., 21 Madison St., Rochester, N. Y. (15).
Kinner, Dr. Hugo, 1517 South Seventh St., St. Louis, Mo. (21).
Kinnicutt, Leonard P., Worcester, Mass. (28).
Kirkland, Thomas, Toronto, Ontario, Can. (31).
Kirkpatrick, James A., 1138 Girard St., Philadelphia, Pa. (7).
Kite, Thomas, 27 Johnson Building, Cincinnati, Ohio (5).
Kleinschmidt, John L. (29).
Knapp, Frederick N., Plymouth, Mass. (19).
Knight, Prof. Charles M., Akron, Ohio (29).
Knight, Miss Elizabeth G., 125 East 54th St., New York, N. Y. (31).
Knight, Walter H., Washington, D. C. (30).
Knowlton, Wm. J., 168 Tremont St., Boston, Mass. (29).
Kocsis, Alexander, Tullahoma, Tenn. (26).
Kunz, G. F., care Tiffany & Co., New York, N. Y. (29).

Lacoe, R. D., Pittston, Pa. (31).
Ladd, Wm. H., Chauncy Hall School, 259 Boylston St., Boston, Mass. (29).
Laflamme, Prof. J. C. K., Laval Univ., Quebec, Can. (29).
Laist, Otto, Cincinnati, Ohio (30).
Lakes, Prof. Arthur, State School of Mines, Golden, Col. (26).
Lamb, Mrs. Martha J., Coleman House, New York, N. Y. (29).
Lamborn, Robert H., 47 William St., New York, N. Y. (28).
Landreth, Prof Olin H., Vanderbilt Univ., Nashville, Tenn. (28).
Lane, Jonathan A., Boston, Mass. (29).
Langenbeck, Karl W., cor. 7th and Vine Sts., Cincinnati, Ohio (30).
Larimer, James E., Lawrenceburg, Ind. (30).

Larkin, Edgar L., New Windsor, Mercer Co., Ill. (28).
Larkin, Frederick, M. D., Randolph, N. Y. (28).
Lathrop, J. C., Bridgeport, Conn. (31).
Latimer, Charles, Chief Eng. N. Y. P. and O. R. R., Cleveland, Ohio (31).
Latour, Major L. A. Huguet, 36 McGill College Ave., Montreal, Can. (31).
Laudy, Louis H., School of Mines, Columbia College, New York, N. Y. (28).
Lawrence, Hon. Edw., Pres't Bunker Hill National Bank, Charlestown, Mass. (18).
Laws, Miss Annie, 100 Dayton St., Cincinnati, Ohio (30).
Lawson, George, LL. D., Halifax, N. S. (31).
Lea, John McCormick, Nashville, Tenn. (26).
Learned, Rev. J. C., 1748 Waverly Place, St. Louis, Mo. (27).
Leavenworth, Francis P., Mt. Lookout, Hamilton Co., Ohio (30).
Le Boutillier, Charles A., M. D., Cincinnati, Ohio (30).
Ledyard, L. W., Cazenovia, Madison Co., N. Y. (29).
Lee, Prof. Leslie A., Bowdoin College, Brunswick, Me. (29).
Lee, Prof. Wm., 2111 Penna. Avenue, Washington, D. C. (29).
Leete, James M., M.D., 2912 Washington Avenue, St. Louis, Mo. (27).
Leffmann, Henry, M. D., S. E. cor. 18th and Locust Sts., Philadelphia, Pa. (31).
Lehman, G. W., Ph. D., 57 So. Gay St., Baltimore, Md. (30).
Leland, Eugene R., P. O. Box 2364, New York, N. Y. (25).
Lemp, Wm. J., cor. Cherokee and 2nd Carondelet Ave., St. Louis, Mo. (27).
Lennon, William H., Brockport, N. Y. (31).
Leslie, Dr. James, 278 7th St., Cincinnati, Ohio (30).
Lesley, Joseph, 233 South Fourth St., Philadelphia, Pa. (8).
Letchworth, Josiah, Buffalo, N. Y. (25).
L'Etoile, Joseph, Dept. of Interior, Quebec, Can. (31).
Letterman, Geo. W., Allenton, Mo. (27).
Lewis, David S., 609 Commercial St., Boston, Mass. (29).
Lewis, Elias, jr., 111 St. Mark's Ave., Brooklyn, N. Y. (23).
Libbey, Joseph, Georgetown, D. C. (31).
Libbey, William, jr., Princeton, N. J. (29).
Liebig, Dr. G. A., 87 Exchange Place, Baltimore, Md. (30).
Linderman, Dr. G. B., Bethlehem, Pa. (28).
Lindsley, Adrian Van S., Sec'y Univ. of Nashville, Nashville, Tenn. (26).
Lippman, Morris J., care Graff, Bennett & Co., St. Louis, Mo. (27).
Little, William, Montreal, Can. (31).
Livermore, Mrs. M. A. C., 24 North Avenue, Cambridge, Mass. (29).
Lloyd, John U., Cincinnati, Ohio (30).
Lloyd, Mrs. Rachel, Louisville, Ky. (31).
Locke, W. M., Agt. L. and N. R. R., Marshalls Ferry, White Co., Ill. (29).
Logan, John, M. D., 141 E. 15th St., New York, N. Y. (29).
Lomb, Carl. F., Rochester, N. Y. (29).

Loomis, Horatio, Burlington, Vt. (31).
Loomis, Leverett M., Chester, S. C. (28).
Loud, Prof. Frank H., Colorado Springs, Col. (29).
Lounsbury, Prof. Thos. R., New Haven, Conn. (31).
Lovell, John W., New York, N. Y. (31).
Lovewell, Prof. Joseph T., Washburn College, Topeka, Kansas (25).
Low, Seth, 31 Burling Slip, New York, N. Y. (29).
Lowe, C. B. F., Augusta, Ga. (30).
Lowe, Nathl. M., 103 Court St., Boston, Mass. (29).
Lowell, Aug., 60 State St., Boston, Mass. (29).
Lowman, Oscar, 66 W. 8th St., Cincinnati, Ohio (30).
Lowrie, J. R., Warriorsmark, Pa. (29).
Lufkin, Albert, Newton, Iowa (31).
Lugo, Dr. Orazio, Room 2, University Building, Washington Sq., New York, N. Y. (29).
Lunn, William, Montreal, Can. (31).
Lyford, Prof. Moses, Waterville, Me. (22).
Lyman, Benj. Smith, Northampton, Mass. (15).
Lyman, Henry H., 74 McTavish St., Montreal, Can. (29).
Lynch, W. H., Danville, P. Q., Can. (31).

MacArthur, Charles L., Troy, N. Y. (19).
MacCauley, Rev. Clay, Bureau of Ethnology, Washington, D. C. (29).
Mack, William, M.D., Salem, Mass. (21).
Mackenzie, Dr. J. C., 163 W. 7th St., Cincinnati, Ohio (30).
Mackintosh, James B., School of Mines, Columbia Coll., New York, N. Y. (27).
Macomber, Albert E., Toledo, Ohio (30).
Macomber, Isaac B., Portsmouth, R. I. (29).
Macy, Arthur, C. E., 609 Lexington Avenue, New York, N. Y. (26).
Mager, W., 57 So. Gay St., Baltimore, Md. (30).
Maine, Henry C., Rochester, N. Y. (29).
Major, Dr. George W., Montreal, Can. (31).
Mallinckrodt, Edw., P. O. Sub-Station A, St. Louis, Mo. (29).
Mann, Miss M. P., Troy Seminary, Troy, N. Y. (28).
Manning, Richard C., Salem, Mass. (29).
Manning, Richard H., Brooklyn, N. Y. (29).
Manning, W. H., Reading, Mass. (31).
Mansfield, J. M., Mt. Pleasant, Iowa (25).
Marble, J. Russel, Worcester, Mass. (31).
Marble, Miss Sarah, Woonsocket, R. I. (29).
Marcy, Henry O., M. D., 690 Main St., Cambridge, Mass. (28).
Marler, George L., Montreal, Can. (31).
Marsden, Samuel, 907 Clay Ave., St. Louis, Mo. (27).
Marshall, William I., Fitchburg, Mass. (27).
Martin, Prof. B. N., 236 W. 4th St., New York, N. Y. (23).
Martin, Prof. William J., Davidson College, N. C. (31).

Metcalf, Caleb B., Highland Military Academy, Worcester, Mass. (20).
Martindale, Isaac C., Camden, N. J. (26).
Marvin, Hon. Wm., Skaneateles, N. Y. (28).
Mason, Norman N., Providence, R. I. (29).
Mason, Dr. William P., Troy, N. Y. (31).
Mathews, Robert, 28 Spring St., Rochester, N. Y. (25).
Matlack, Charles, Germantown, Pa. (27).
Mattison, Joseph G., Hastings-on-Hudson, N. Y. (30).
Maxwell, Geo. M., Wyoming, Hamilton Co., Ohio (30).
May, Miss Abby W., 3 Exeter St., Boston, Mass. (29).
May, Addison, West Chester, Pa. (29).
May, John J., Box 5213, Boston, Mass. (29).
Mayer, William G., Cincinnati, Ohio (30).
Mays, Dr. Thos. J., Upper Lehigh, Pa. (29).
McAdams, Wm., jr., Otterville, Ill. (27).
McAlpin, William, 133 Broadway, Cincinnati, Ohio (30).
McChesney, Charles E., M. D., U. S. A., Fort Bennett, Dakota Terr. (30).
McClellan, Miss Fanny, Oxford, Ohio (30).
McClung, Calvin M., Knoxville, Tenn. (26).
McCook, Rev. Henry C., Philadelphia, Pa. (29).
McCutchen, Aug. R., La Fayette, Ga. (25).
McDonald, Prof. Marshall, Lexington, Va. (29).
McFadden, Thos., Otterbein Univ., Westerville, Ohio (30).
McGuire, Joseph D., M. D., Ellicott City, Md. (30).
McInnis, Prof. Louis L., College Station, Texas (31).
McLachlan, J. S., 480 St. Paul St., Montreal, Can. (31).
McLarty, James M., care Alex. Moore, No. 3 School St., Boston, Mass. (31).
McLennan, Hugh, Montreal, Can. (31).
McMaster, Æneus, Scotstown, P. Q., Can. (31).
McNair, F. V., Commander U. S. N., Naval Academy, Annapolis, Md. (28).
McNeal, Albert T., Bolivar, Tenn. (26).
McWhorter, Tyler, Aledo, Ill. (20).
Mead, Walter H., 67 Wall St., New York, N. Y. (29).
Means, Mrs. W. E. C., Yellow Springs, Greene Co., Ohio (30).
Meehan, Mrs. Thos., Germantown, Pa. (29).
Mehner, Edwin L., 34 E. Pearl St., Cincinnati, Ohio (30).
Meier, Theodore G., St. Louis, Mo. (27).
Meiners, Prof. Hermann, St. Xavier College, Cincinnati, Ohio (30).
Mendenhall, R. J., Minneapolis, Minn. (31).
Merchant, Richard V., East Weymouth, Mass. (29).
Merkel, G. H., M. D., 322 Shawmut Avenue, Boston, Mass. (29).
Merrick, Hon. Edwin T., New Orleans, La. (29).
Merriman, Prof. Geo. B., Rutgers College, New Brunswick, N. J. (29).
Merryman, Capt. James H., U. S. R. M., 3 Bowling Green, New York, N. Y. (31).
Merryweather, Geo. N., cor. 6th and Race Sts., Cincinnati, Ohio (30).

Metz, Charles L., M. D., Madisonville, Hamilton Co., Ohio (30).
Mew, Wm. M., Army Med. Mus., Washington, D. C. (29).
Michels, John, Box 3230, New York, N. Y. (29).
Miles, Prof. Manly, Mass. Agric. Coll., Amherst, Mass. (29).
Miller, Dr. Chas. A., Longview Asylum, Carthage, Hamilton Co., Ohio (30).
Miller, D. B., 604 Greenup St., Covington, Ky. (30).
Miller, Edgar G., 279 Baltimore St., Baltimore, Md. (29).
Miller, Geo. M., 20 Broad St., New York, N. Y. (29).
Miller, John A., Cairo, Ill. (22).
Miller, S. A., Cincinnati, Ohio (26).
Mills, James, M. A., Guelph, Ontario, Can. (31).
Mills, T. Wesley, Montreal, Can. (31).
Mills, William H., Hamilton, Ontario, Can. (31).
Minot, Francis, M. D., 65 Marlborough St., Boston, Mass. (29).
Mitchell, Geo. O., Hanover, N. H. (29).
Mixter, Calvin S., Census Bureau, Washington, D. C. (31).
Moat, Robert, Montreal, Can. (31).
Moffatt, George T., M. D., D. M. D., 132 Boylston St., Boston, Mass. (25).
Mohr, C. A., Mobile, Ala. (30).
Mohr, Charles, Mobile, Ala. (30).
Mohr, Miss Emily, Cincinnati, Ohio (30).
Mohr, Miss Marie, Cincinnati, Ohio (30).
Mohr, Paul, Cincinnati, Ohio (30).
Moliner, Adolfo, 530 Cerro, Havana, Cuba (28).
Molson, John, Montreal, Can. (31).
Molson, John H. R., Montreal, Can. (31).
Montgomery, Henry, 536 Ontario St., Toronto, Can. (29).
Mooar, Daniel, Keokuk, Iowa (27).
Moody, Mrs. Mary B., M.D., 187 N. Pearl St., Buffalo, N. Y. (25).
Moore, Alex., 3 School St., Boston, Mass. (29).
Moore, Alvin H., Magog, P. Q., Can. (31).
Moore, E. C., care Tiffany & Co., New York, N. Y. (30).
Morehouse, George W., Wayland, Steuben Co., N. Y. (24).
Morey, Rev. A. B., Cincinnati, Ohio (30).
Morgan, James E., M. D., 905 E St., N. W., Washington, D. C. (30).
Morgan, Mrs. Mary E., 95 So. Fitzhugh St., Rochester, N. Y. (31).
Morgan, Wm. F., 1 East 40th St., New York, N. Y. (27).
Morison, Dr. N. H., Provost of Peabody Institute, Baltimore, Md. (17).
Morrice, D., Montreal, Can. (31).
Morrison, Nathan J., Springfield, Mo. (29).
Morse, Charles J., Youngstown, Ohio (31).
Morse, H. L., M. D., Park Square, Boston, Mass. (31).
Morse, Mrs. Mary J., 57 Jackson St., Lawrence, Mass. (29).
Mortimer, Capt. John H., care of F. Habirshaw, 113 Maiden Lane, New York, N. Y. (31).
Morton, Miss Anna J., 1800 Michigan Ave., Chicago, Ill. (31).

Moser, Lieut. Jeff. F., U. S. N., Slatington, Pa. (28).
Moses, Prof. Thos. F., Urbana University, Urbana, Ohio (25).
Mosgrove, Samuel M., M. D., Urbana, Ohio (30).
Mott, Dr. H. A., jr., 105 Water St., New York, N. Y. (27).
Mowry, Wm. A., 49 Snow St., Providence, R. I. (29).
Mühlberg, William, Race and Elder Sts., Cincinnati, Ohio (30).
Muir, John, Martinez, Cal. (22).
Muir, William, 1395 St. Catherine St., Montreal, Can. (31).
Munn, John P., M. D., 18 West 58th St., New York, N. Y. (31).
Munro, Rev. A. H., 340 Bleury St., Montreal, Can. (31).
Murdoch, John, 195 Walnut Avenue, Roxbury, Mass. (29).
Murdock, Lieut. J. B., U. S. N., Annapolis, Md. (28).
Murphy, Edward, 405 St. Paul St., Montreal, Can. (31).
Murphy, Patrick J., Columbia Hospital, Washington, D. C. (30).
Murray, Rev. Dr. J. Clark, 111 Mackay St., Montreal, Can. (31).
Murtfeldt, Miss Augusta, Kirkwood, Mo. (29).
Mussey, Rev. Ch. F., Cincinnati, Ohio (30).
Myers, John A., Bethany, West Va. (30).
Myricke, Lockwood, Pacific Guano Co., Boston, Mass. (30).

Nachtrieb, Henry F., 15 North St., St. Paul, Minn. (29).
Nagel, Herman, M. D., 2044 Lafayette Avenue, St. Louis, Mo. (30).
Neale, Arthur T., New Brunswick, N. J. (29).
Nelson, Anson, City Treasurer, Nashville, Tenn. (26).
Nelson, Richard, Cincinnati, Ohio (30).
Nesmith, Henry E., jr., 28 South St., New York, N. Y. (30).
Nettleroth, Henry, C. E., Louisville, Ky. (29).
Nettleton, A. B., Minneapolis, Minn. (31).
Nettleton, Chas., Room 20, 117 Broadway, New York, N. Y. (30).
Newington, Harry M., 1603 Washington Ave., St. Louis, Mo. (31).
Newton, Rev. John, Mary Esther, West Fla. (7).
Nichols, H. E., Lieut. Comdr. U. S. N., Box 967, San Francisco, Cal. (29).
Nicholson, Prof. Hunter, Knoxville, Tenn. (26).
Nolan, Edw. J., M. D., Philadelphia, Pa. (29).
Norman, Geo. H., C. E., Newport, R. I. (29).
Norton, Lewis M., Ph.D., Mass. Inst. of Technology, Boston, Mass. (29).
Norton, Oliver D., M. D., Cincinnati, Ohio (31).
Nuttall, L. W., Nuttallburg, Fayette Co., West Va. (29).

Ober, Frederick A., Beverly, Mass. (31).
Oliver, Prof. Marshal, U. S. N., Naval Acad. Annapolis, Md. (31).
Olmsted, Fred. Law, 209 W. 46th St., New York, N. Y. (22).
Ordway, Mrs. John M., Mass. Inst. Technology, Boston, Mass. (29).
Orm, John, Paducah, McCracken Co., Ky. (27).
Osborn, Francis A., 43 Milk St., Boston, Mass. (29).
Osborn, Henry F., S. D., Garrisons, N. Y. (29).

Osborn, Henry L., Curator, Mus. of Wesleyan Univ., Middletown, Conn. (29).
Osborne, Mrs. Ada M., Waterville, Oneida Co., N. Y. (19).
Osborne, Amos O., Waterville, Oneida Co., N. Y. (19).
Osbun, Prof. Isaac J., Salem, Mass. (29).
Osgood, Joseph B. F., Salem, Mass. (31).
Otis, J. Sanford, Cincinnati, Ohio (30).
Overstolz, Hon. Henry, Mayor of St. Louis, St. Louis, Mo. (27).
Owen, Frank A., 304 North St., Burlington, Vt. (31).
Owens, Mary E., cor. 7th and John Sts., Cincinnati, Ohio (30).

Packer, Hon. Harry E., Box 228, Mauch Chunk, Carbon Co., Pa. (30).
Paddock, John R., Hoboken, N. J. (29).
Paine, Charles, 15 Broad St., New York, N. Y. (22).
Paine, Sidney B., P. O. Box 338, Fall River, Mass. (30).
Palmer, Miss Alice W., Bellevue St., Roxbury, Mass. (27).
Palmer, Rev. Benj. M., Box 1628, New Orleans, La. (21).
Palmer, Dr. Edward, care Peabody Museum, Cambridge, Mass. (22).
Palmer, Miss Mary T., Bellevue St., Roxbury, Mass. (30).
Parsons, Henry B., 36 East 30th St., New York, N. Y. (30).
Parsons, S. S., Lodi, Medina Co., Ohio (30).
Patrick, Dr. J. J. R., Belleville, St. Clair Co., Ill. (27).
Patton, William H., Waterbury, Conn. (29).
Paul, Caroline A., M.D., Vineland, Cumberland Co., N. J. (23).
Payne, A. R., Price's Hill, Cincinnati, Ohio (30).
Payne, Prof. Wm. W., Carleton College, Northfield, Minn. (30).
Peabody, Prof. S. H., Regent Illinois Industrial Univ., Champaign, Ill. (30).
Peaslee, Prof. John B., Supt. Public Schools, Cincinnati, Ohio (30).
Peck, W. A., C. E., Surveyor General's Office, Denver, Col. (19).
Peelor, David, Johnstown, Cambria Co., Pa. (30).
Peirce, Benj. O., jr., Beverly, Mass. (29).
Peirce, Prof. C. S., Johns Hopkins Univ., Baltimore, Md. (30).
Peirce, Cyrus N., D.D.S., 1415 Walnut St., Philadelphia, Pa. (31).
Peirce, Prof. James M., Cambridge, Mass. (28).
Pennock, Edw., 924 Chestnut St., Philadelphia, Pa. (29).
Perkins, Arthur, 49 Woodland St., Hartford, Conn. (31).
Perry, Hon. Aaron F., Cincinnati, Ohio (30).
Peter, Alfred M., Lexington, Ky. (29).
Peters, Judge Thos. M., Moulton, Ala. (29).
Pettee, Prof. C. H., Hanover, N. H. (31).
Phelps, A. M., M. D., Chateaugay, N. Y. (31).
Phelps, Mrs. Almira Lincoln, Baltimore, Md. (13).
Phelps, George, Nashua, N. H. (31).
Philbrick, Edw. S., Brookline, Mass. (29).
Phillips, John C., cor. Berkeley & Marlborough Sts., Boston, Mass. (29).
Phin, John, Ed. Am. Journ. Micros., 14 Dey St., New York, N. Y. (25).

Pickering, Wm. H., 84 Mt. Vernon St., Boston, Mass. (29).
Pickett, Dr. Thos. E., Maysville, Mason Co., Ky. (25).
Pignolet, Louis H., 104 Famille St., Montreal, Can. (31).
Pike, J. W., Vineland, N. J. (29).
Pike, W. A., 521 4th St., S. E., Minneapolis, Minn. (29).
Pillsbury, John H., 34 Avon Place, Springfield, Mass. (23).
Pinkerton, T. H., M.D., Oakland, Alameda Co., Cal. (27).
Pirz, Anthony, Long Island City, N. Y. (29).
Pitkin, Lucius, 434 Madison Avenue, New York, N. Y. (29).
Pitman, Chas. B., Somerville, Mass. (29).
Pitman, Prof. Stephen M., College Hill, Mass. (29).
Pitt, Prof. William H., 138 College St., Buffalo, N. Y. (25).
Plympton, Gilbert M., 245 Broadway, New York, N. Y. (29).
Porteous, John, 26 Prince Arthur St., Montreal, Canada (22).
Porter, John B., Glendale, Hamilton Co., Ohio (30).
Potter, Elizabeth B., Hamilton, Butler Co., Ohio (30).
Potter, Rev. Henry C., 804 Broadway, New York, N. Y. (29).
Potter, Stephen H., Hamilton, Butler Co., Ohio (30).
Power, Prof. Frederick B., 145 N. 10th St., Philadelphia, Pa. (31).
Prang, Louis, Boston, Mass. (29).
Prescott, Wm. I., 264 Race St., Cincinnati, Ohio (30).
Preston, Prof. W. C., Laboratory, Iowa State Univ., Iowa City, Iowa (21).
Prince, Gen. Henry, U. S. A., Eastport, Me. (22).
Prince, L. M., 148 W. 4th St., Cincinnati, Ohio (30).
Prosser, Col. Wm. F., Seattle, King Co., Washington Terr. (26).
Proudfoot, Alex., M. D., 2 Phillips Place, Montreal, Can. (31).
Pruyn, John V. L., jr., Albany, N. Y. (29).
Pulsifer, Sidney, 405 South 42nd St., Philadelphia, Pa. (21).
Purinton, Prof. D. B., Morgantown, W. Va. (31).
Purinton, Prof. George D., Des Moines, Iowa (31).
Putnam, Chas. P., M. D., 63 Marlborough St., Boston, Mass. (28).

Quick, Edgar R., Brookville, Franklin Co., Ind. (30).

Rains, Geo. W., M. D., LL. D., Augusta, Ga. (29).
Ramsay, Robert A., Montreal, Can. (31).
Rand, C. F., M.D., Batavia, N. Y. (27).
Randolph, A. F., Fredericton, N. B. (29).
Rapp, Geo. W., 5th and Walnut Sts., S. E. C., Cincinnati, Ohio (30)
Rasin, R. W., Box 715, Baltimore, Md. (30).
Rathbun, Frank R., 40 Franklin St., Auburn, N. Y. (29).
Rattermann, H. A., 12th and Walnut Sts., Cincinnati, Ohio (30).
Redding, Thos. B., Ph. D., New Castle, Ind. (29).
Reed, C. A. L., Hamilton, Ohio (30).
Reed, Edwin, 178 Devonshire St., Boston, Mass. (29).
Reed, Dr. R. C. S., Jones' Station, Butler Co., Ohio (30).

Renninger, John S., M. D., Minneota, Minn. (31).
Rice, Rev. Wm., D. D., Springfield, Mass. (28).
Richards, Edgar, Dep't of Agric., Washington, D. C. (31).
Richardson, Clifford, Dept. of Agric., Washington, D. C. (30).
Richardson, F. C. A., Nat. Stock Yards, E, St. Louis, Mo. (20).
Richardson, Tobias G., M. D., New Orleans, La. (30).
Richmond, Adelbert G., Canajoharie, N. Y. (30).
Rickard, Wm. T., Assay Office, Tucson, Arizona Terr. (29).
Rideout, Bates S., Lewiston, Me. (31).
Ridler, C. E., Kingston, Mass. (29).
Riggs, Geo. W., jr., 82 Cathedral St., Baltimore, Md. (26).
Riley, Mrs. Susan, Mobile, Ala. (31).
Rivera José de, Inwood-on-the-Hudson, New York, N. Y. (29).
Robbins, E. P., Cincinnati, Ohio (30).
Robbins, Miss Ida F., Arlington, Mass. (31).
Robbins, W. K., Mass. Inst. of Technology, Boston, Mass. (29).
Roberts, Thomas S., 27 North 8th St., Minneapolis, Minn. (31).
Robertson, Andrew, Montreal, Can. (31).
Robertson, Col. Robert S., Fort Wayne, Ind. (20).
Robertson, Thomas D., Rockford, Ill. (10).
Robeson, Henry B., Comd'r U. S. N., Naval Acad., Annapolis, Md. (29).
Robins, Sampson P., LL. D., Hochelaga, P. Q., Can. (31).
Robinson, Prof. Franklin C., Brunswick, Me. (29).
Robinson, Prof. Otis Hall, Rochester, N. Y. (23).
Robinson, Prof. S. W., Univ. of Ohio, Columbus, Ohio (30).
Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28).
Roeder, Prof. F. A., Ohio Medical College, Cincinnati, Ohio (30).
Rogers, Dr. H. Raymond, Dunkirk, N. Y. (30).
Roots, G. Y., N. W. cor. 4th and Walnut Sts., Cincinnati, Ohio (30).
Rosenspitz, Dr. Alexander, Rabbi, Lock Box 430, Portland, Oregon (26).
Ross, Denman Waldo, Ph. D., Cambridge, Mass. (29).
Ross, M. D., Boston, Mass. (29).
Roy, Rev. James, Cobourg, Ont., Can. (31).
Royce, C. C., Box 585, Washington, D. C. (30).
Rumsey, Bronson C., 58 & 60 Exchange St., Buffalo, N. Y. (15).
Rupp, Wm., 961 3d Ave., New York, N. Y. (27).
Russell, Dr. Linus E., Springfield, Ohio (30).
Russell, W. H. H., S. W. cor. Fifth and Olive streets, St. Louis, Mo. (24).
Rust, Horatio N., Pasadema, Los Angeles Co., Cal. (26).

Sabin, Prof. Alvah H., 457 Main St., Burlington, Vt. (31).
Safford, Charles W., Rutland, Vt. (26).
Safford, Dr. Mary J., 308 Columbus Ave., Boston, Mass. (31).
Sage, John H., Portland, Conn. (23).
Salisbury, Stephen, jr., Worcester, Mass. (29).
Salmon, Daniel E., Dep't of Agric., Washington, D. C. (31).
Salter, Richard P., 18 Wall St., New York, N. Y. (30).

Sanborn, Prof. J. W., Univ. of Missouri, Hanover, N. H. (31).
Sander, Dr. Enno, St. Louis, Mo. (27).
Sanders, Benjamin D., Wellsburg, Brooke County, W. Va. (19).
Sargent, Fred'k Leroy, care B. S. Pray & Co., 80 State St., Boston, Mass. (29).
Sattler, Dr. Robert, 64 W. 7th St., Cincinnati, Ohio (30).
Saville, James H., 342 D St., N. W., Washington, D. C. (29).
Sayre, Robert H., Bethlehem, Pa. (28).
Scammon, J. Young, Chicago, Ill. (17).
SCHAFFER, CHAS., M. D., 1309 Arch St., Philadelphia, Pa. (29).
Schaffranck, Dr. A., Box 424, Wheeling, W. Va. (27).
Schaub, Julius W., Ass't Eng'r, Baltimore Bridge Co., St. Louis, Mo. (27).
Schmid, Dr. H. Ernest, White Plains, N. Y. (25).
Schmidt, Dr. H. D., 263½ Canal St., New Orleans, La. (29).
Schöney, Dr. L., 170 E. 71st St., New York, N. Y. (29).
Schuermann, W. H., Cincinnati, Ohio (30).
Schultz, Carl H., 860 Broadway, New York, N. Y. (29).
Schwarz, E. A., U. S. Dept. of Agric., Washington, D. C. (29).
Scott, Aug. E., Lexington, Mass. (29).
Scott, Martin P., M. D., Blacksburg, Va. (31).
Scoville, Dr. S. S., Lebanon, Ohio (30).
Scribner, the Hon. G. Hilton, Yonkers, N. Y. (30).
Scudder, John M., M. D., Cincinnati, Ohio (30).
Seagrave, Frank E., Providence, R. I. (28).
Seargeant, L. J., Montreal, Can. (31).
Secrist, Dr. H. C., 540 Race St., Cincinnati, Ohio (30).
Seeley, Chas. A., 714 Lexington Ave., New York, N. Y. (18).
Seiler, Prof. Jacob F., Harrisburg, Pa. (30).
Selfridge, J. R., Lieut. U. S. N., Navy Dept., Washington, D. C. (29).
Senecal, L. A., 17 Place d'Armes Square, Montreal, Can. (31).
Sennett, George B., Meadville, Pa. (31).
Seymour, Prof. William P., 105 Third St., Troy, N. Y. (19).
Seyms, George H., care of Seyms & Co., Hartford, Conn. (31).
Shackelford, Mrs. Elizabeth P., Saratoga Springs, N. Y. (28).
Shackelford, G. R. P., Saratoga Springs, N. Y. (29).
Shackelford, Rev. J. W., 111 E. 82nd St., New York, N. Y. (29).
Share, William W., 336 Navy St., Brooklyn, N. Y. (31).
Sharples, Stephen P., 114 State St., Boston, Mass. (29).
Shaw, Cyrus, Mountainville P. O., N. Y. (29).
SHEAFER, A. W., Pottsville, Pa. (28).
Sheafer, Walter S., Pottsville, Pa. (25).
Shearer, James, Montreal, Can. (31).
Shearer, John S., Montreal, Can. (31).
Shepard, Rev. Morrill A., Lebanon, St. Clair Co., Ill. (30).
Shepard, William A., Saratoga Springs, N. Y. (28).
Shepherd, F. A., 25 North Vine Street, Nashville, Tenn. (26).
Sheppard, Sam'l A. D., Boston, Mass. (29).

Sherman, Prof. F. A., Hanover, N. H. (29).
Sherman, Lewis, M. D., Milwaukee, Wis. (30).
Short, Sidney H., Ohio State Univ., Columbus, Ohio (28).
Shorten, John W., 191 W. 4th St., Cincinnati, Ohio (30).
Shultz, Charles S., Hoboken, N. J. (31).
Sibley, Miss M. F., East Walnut Hills, Grandin Road, Cincinnati, Ohio (30).
Silliman, Wyllis A., Clarkson, N. Y. (31).
Simon, Prof. Wm., College of Pharmacy, Baltimore, Md. (29).
Simonds, Frederic W., 68 Fulton St., Syracuse, N. Y. (25).
Simpson, E., Commodore U. S. N., U. S. Navy Yard, League Island, Pa. (28).
Skinner, A. G., M. D., Youngstown, Niagara Co., N. Y. (29).
Slade, Elisha, Somerset, Mass. (29).
Smead, Miss Marian M., Batavia, N. Y. (31).
Smiley, Charles W., Smithsonian Institution, Washington, D. C. (28).
Smith, Mrs. A. B., care of Hon. J. Gregory Smith, St. Albans, Vt. (31).
Smith, Benj. G., Cambridge, Mass. (29).
Smith, Prof. C. Alfred, Hulton, Allegheny Co., Pa. (30).
Smith, Prof. Charles A., Washington Univ., St. Louis, Mo. (27).
Smith, Edwin, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (30).
Smith, Goldwin, The Grange, Toronto, Can. (29).
Smith, Dr. H. A., 286 Race St., Cincinnati, Ohio (30).
Smith, Henry L., 149 Broadway, New York, N. Y. (26).
Smith, Mrs. Henry L., 149 Broadway, New York, N. Y. (26).
Smith, Henry T., Buffalo, N. Y. (25).
Smith, Herbert E., M. D., Instructor of Chem. Med. Dep't, Yale College, New Haven, Conn. (31).
Smith, Prof. Herbert S. S., Lawrence, Kan. (29).
Smith, Herbert W., Academy of Sciences, Box 2621, St. Paul, Minn. (26).
Smith, Mrs. J. Lawrence, Louisville, Ky. (26).
Smith, J. W., M.D., Charles City, Iowa (21).
Smith, Miss Jennie, Peabody Mus., Cambridge, Mass. (29).
Smith, S. Hanbury, M. D., New York, N. Y. (29).
Smith, Prof. Thomas B., Louisiana, Mo. (30).
Smith, Thomas H., 161 La Salle St., Chicago, Ill. (31).
Smith, Wm. Sooy, C. E., Maywood, Ill. (29).
Smucker, Isaac, Newark, Ohio (29).
Smyth, Prof. Jas. D., Burlington, Iowa (28).
Snelling, Geo. H., Hotel Berkeley, Boston, Mass. (29).
Snyder, Henry, jr., Institute for the Blind, Columbus, Ohio (30).
Soley, John C., Lieut. U. S. N., Navy Dept., Washington, D. C. (29).
Solomons, Miss Augusta I., Sumter, S. C. (31).
Solomons, Miss Kate C., Sumter, S. C. (31).
Solomons, Miss Maude C., Sumter, S. C. (31).
Soper, A. W., St. Louis, Mo. (27).

Spafford, F. A., M. D., Ludlow, Vt. (29).
Spear, Rev. C. V., Pittsfield, Mass. (29).
Speck, Hon. Charles, St. Louis, Mo. (27).
Speir, Francis, jr., South Orange, N. J. (29).
Spence, Mrs. E. Jane, Springfield, Ohio (30).
SPRAGUE, C. H., Malden, Mass. (29).
Sprague, Chas. J., Globe Nat. Bank, Boston, Mass. (29).
Sprague, Frank J., U. S. N., care J. M. Sprague, 1249 Broadway, New York, N. Y. (29).
Stæbner, Fred W., Westfield, Mass. (28).
Starr, Charles S., M. D., Rochester, N. Y. (31).
Stearns, Eben S., Chancellor Univ. of Nashville, Nashville, Tenn. (26).
Stearns, Josiah A., Ph. D., Boston, Mass. (29).
Stearns, Silas, Assistant U. S. Fish Commission, Pensacola, Fla. (28).
Stearns, Winfrid Alden, Amherst, Mass. (23).
Stebbins, James H., jr., 117 Pearl St., New York, N. Y. (31).
Sterling, Prof. Theodore, Gambier, Ohio (30).
Sterling, W. S., Clifton, Hamilton Co., Ohio (30).
Stern, David, care B. Stern, Cincinnati, Ohio (30).
Stevens, Geo. T., M. D., Albany, N. Y. (28).
Stevens, R. P., Jamaica, N. Y. (18).
Stevenson, James, Ass't Ethnologist, Washington, D. C. (29).
Stevenson, W. G., M. D., Poughkeepsie, N. Y. (28).
Stillman, W. O., M. D., Proctorsville, Vt. (28).
Stillwell, Charles M., Box 1261, New York, N. Y. (30).
Stimpson, Thomas M., Peabody, Mass. (18).
Stimson, E. R., Toronto, Ontario (31).
St. John, Jas. A., St. Louis, Mo. (27).
Stockbridge, Horace E., Amherst, Mass. (31).
Stockbridge, Levi, Amherst, Mass. (31).
Stoddard, John T., Ph. D., Northampton, Mass. (29).
Stodder, Charles, 131 Devonshire St., Boston, Mass. (29).
Stone, Mrs. Ellen M. P., Providence, R. I. (31).
Stone, Henry, New York, N. Y. (31).
Stone, Rev. James A. B., Detroit, Mich. (31).
Stone, Lincoln R., M. D., Newton, Mass. (31).
Stone, Miss Mary H., Salem, Mass. (25).
Storer, Dr. D. H., Boston, Mass. (1).
Stowell, John, 48 Main St., Charlestown, Mass. (21).
Stowell, Prof. T. B., Cortland, N. Y. (28).
Studer, Jacob H., Tribune Building, New York, N. Y. (31).
Stunz, Prof. C. R., Cincinnati, Ohio (29).
Sullivan, J. A., Malden, Mass. (27).
Swasey, Oscar F., M.D., Beverly, Mass. (17).

Taft, Alphonso, Cincinnati, Ohio (30).
Tatum, Joseph T., 221 Chestnut St., St. Louis, Mo. (27).

Taylor, Frederick W., Smithsonian Institution, Washington, D. C. (31).
Taylor, Comdr. H. C., U. S. N., care U. S. Navy Dept., Washington, D. C. (30).
Taylor, James C., care A. R. Ledoux & Co., 17 Cedar St., New York, N. Y. (30).
Taylor, Thos., M. D., Dept. of Agric., Washington, D. C. (29).
Tepper, Fred., Box 3331, New York, N. Y. (29).
Terry, James, American Museum Natural History, Central Park (77th St. and 8th Ave.), New York, N. Y. (28).
Theobald, Albert G. R., Forest Dept., Anamalai, Coimbatore District, India (27).
Thomas, Prof. Cyrus, Carbondale, Ill. (30).
Thomas, F. Wolferstan, Montreal, Can. (31).
Thompson, Prof. Chas. O., Terre Haute, Ind. (29).
Thompson, Daniel G., 29 William St., New York, N. Y. (29).
Thompson, David D., Methodist Book Concern, Cincinnati, Ohio (30).
Thompson, Harvey M., Lombard, Du Page Co., Ill. (17).
Thompson, J. E., Spalding, Mich. (31).
Thompson, Miss Marie N., 323 Elm St., Cincinnati, Ohio (30).
Thompson, W. W., care Jeffras Seely & Co., Cincinnati, Ohio (30).
Thomson, Prof. A., Ames, Iowa (21).
Thomson, Prof. Henry R., Crawfordsville, Ind. (30).
Thorburn, John, LL. D., Ottawa, Ont., Can. (29).
Thrasher, Allen B., M. D., 94 W. 7th St., Cincinnati, Ohio (30).
Thurber, Miss Elizabeth, Plymouth, Mass. (22).
Tiffany, Asa S., 901 West 5th St., Davenport, Scott Co., Iowa (27).
Tiffin, Mrs. Joseph, jr., cor. Sherbrooke and University Sts., Montreal, Can. (31).
Timby, Theo. R., Nyack, N. Y. (29).
Tittmann, Otto H., U. S. Coast Survey, Washington, D. C. (24).
Tobin, Thomas W., Louisville, Ky. (30).
Todd, Albert, St. Louis, Mo: (27).
Todd, Andrew J., 261 Broadway, New York, N. Y. (29).
Todd, Prof. James E., Tabor, Fremont Co., Iowa (22).
Tolman, James P., 66 Chauncy St., Boston, Mass. (29).
Tomlinson, Dr. J. M., 28 East Ohio St., Indianapolis, Ind. (20).
Tonnelé, Theodore, 48 E. 68th St., New York, N. Y. (29).
Tonry, Prof. Wm. P., Baltimore, Md. (30).
Torrey, Herbert G., U. S. Assay Office, 30 Wall St., New York, N. Y. (31).
Townsend, Franklin, Adj. Gen. State of N. Y., 4 Elk St., Albany, N. Y. (4).
Townsend, Joseph, Durham, Ontario, Can. (31).
Traber, Jacob, Cincinnati, Ohio (30).
Tracy, Will W., Detroit, Mich. (30).
Treadwell, Geo. A., Phœnix, Arizona (29).
Treat, Erastus B., 757 Broadway, N. Y. (29).

Trippe, T. Martin, C. E., Howardsville, San Juan Co., Col. (29).
Troop, James, Agricultural College, Lansing, Mich. (31).
Trottier, A. A., Montreal, Can. (31).
Trowbridge, Mrs. L. H., 266 Woodward Ave., Detroit, Mich. (21).
Trowbridge, Luther H., 266 Woodward Ave., Detroit, Mich. (29).
Trowbridge, S. H., Glasgow, Mo. (27).
Tucker, Willis G., M. D., Albany, N. Y. (29).
Tyler, Edward R., Washington, D. C. (31).
Tyler, John M., Amherst, Mass. (29).

Upham, Dr. J. Baxter, 31 Chestnut St., Boston, Mass. (14).
Upton, Francis R., Box 95, Orange, N. J. (28).
Upton, Winslow, Army Signal Office, Washington, D. C. (29).

Vail, Prof. Hugh D., 1927 Mt. Vernon St., Philadelphia, Pa. (18).
Valentine, Ferdinand C., M. D., 30 West 11th St., New York, N. Y. (31).
Van Brunt, C., Poughkeepsie, N. Y. (28).
Van Tyne, Uriel C., Cincinnati, Ohio (30).
Venable, W. H., Station C, Cincinnati, Ohio (30).
Vermyné, J. J. B., M. D., New Bedford, Mass. (29).
Very, Lt. S. W., care of Navy Dep't, Washington, D. C. (28).
Vieth, William, cor. 8th and Freeman Sts., Cincinnati, O. (28).
Vinton, Mrs. Helena Stallo, Indianapolis, Ind. (30).
Vinton, Lindley, Indianapolis, Ind. (30).
Volney, C. W., Ph. D., 52 Wall St., New York, N. Y. (31).
Voorhees, Chas. H., M. D., Box 92, New Brunswick, N. J. (29).
Vredenburgh, Edw. H., Rochester, N. Y. (29).
Vultë, Hermann T., Ph. B., School of Mines, Columbia College, New York, N. Y. (30).

Wachsmuth, Charles, Burlington, Iowa (30).
Wagner, Ferd., 41 W. 5th St., Cincinnati, Ohio (30).
Wainwright, Ellis, St. Louis, Mo. (29).
Wait, Prof. Chas. E., Rolla, Mo. (29).
Waite, Chas. N., Manchester, N. H. (29).
Wakeman, Thaddeus B., 93 Nassau St., New York, N. Y. (25).
Walcott, Dr. H. P., Cambridge, Mass. (29).
Walker, Dr. G. S., St. Louis, Mo. (27).
Walker, George C., 124 La Salle St., Chicago, Ill. (17).
Walker, N. B., 19 and 21 Cliff St., New York, N. Y. (20)
Wall, John L., 338 Sixth Avenue, New York, N. Y. (27).
Walmsley, W. H., 1016 Chestnut St., Philadelphia, Pa. (28).
Walter, Hugo, Mt. Auburn, Cincinnati, Ohio (30).
Walton, James, Barnesville, Belmont Co., Ohio (30).
Walton, Jos. J., Philadelphia, Pa. (29).
Walworth, Rev. Clarence A., Albany, N. Y. (28).

Walworth, Mrs. Ellen Hardin, Saratoga Springs, N. Y. (28).
Wanless, John, M. D., 88 Union Ave., Montreal, Can. (31).
Ward, J. Langdon, 120 Broadway, New York, N. Y. (29).
Ward, Samuel B., M. D., Albany, N. Y. (29).
Wardwell, George J., Rutland, Vt. (20).
Waring, Col. George E., jr., Newport, R. I. (29).
Warnecke, Prof. Carl, Montreal, Can. (31).
Warner, Hulbert H., Rochester, N. Y. (31).
Warner, Mrs. J. D., 199 Baltic St., Brooklyn, N. Y. (21).
Warnock, James, 33 Warren St., New York, N. Y. (29).
Warren, G. Washington, 54 Devonshire St., Room 1, Boston, Mass. (18).
Warren, Joseph W., M. D., 107 Boylston St., Boston, Mass. (31).
Warren, Samuel D., Boston, Mass. (29).
Warren, Mrs. Samuel D., 67 Mt. Vernon St., Boston, Mass. (29).
Warren, Wm. J., Minneapolis, Minn. (30).
Washburn, John H., Bridgewater, Mass. (31).
Waterhouse, Ai, M. D., Jamestown, N. Y. (29).
Waters, Edwin F., Newton Centre, Mass. (29).
Waters, Geo. F., 8 Beacon St., Boston, Mass. (29).
Waters, Jabez M., 9th and Freeman Sts., Cincinnati, Ohio (30).
Watson, Miss C. A., Salem, Mass. (31).
Watt, David A. P., Montreal, Can. (31).
Webb, Prof. J. Burkitt, Ithaca, N. Y. (31).
Weber, H. C., 17 Mc Lemore St., Nashville, Tenn. (30).
Weed, Walter H., 6 Pine St., New York, N. Y. (31).
Weeden, Hon. Joseph E., Randolph, N. Y. (31).
Weist, J. R., M. D., Richmond, Ind. (29).
Weld, Mrs. Amy T., 119 East 23rd St., New York, N. Y. (28).
Weld, W. G., Box 105, Newport, R. I. (23).
Wells, Mrs. C. F., 753 Broadway, New York, N. Y. (31).
Wells, Rev. Martin L., Aurora, Ind. (30).
Wells, Samuel, 31 Pemberton Square, Boston, Mass. (24).
Wendell, Oliver C., Observatory, Cambridge, Mass. (29).
Wendte, Rev. C. W., Newport, R. I. (29).
West, Arthur W., Salem, Mass. (31).
Wharton, Jos., Box 2786, Philadelphia, Pa. (29).
Wheeler, Prof. C. G., Univ. of Chicago, Chicago, Ill. (18).
Wheeler, T. B., M. D., 714 Craig St., Montreal, Canada (11).
Wheildon, Alice W., Concord, Mass. (31).
Whetstone, John L., N. W. cor. 8th St., and Eggleston Avenue, Cincinnati, Ohio (30).
Whitaker, Prof. Channing, Mass. Inst. Technology, Boston, Mass. (29).
Whitcomb, Wm. W., 106 W. Chester Park, Boston, Mass. (29).
White, Prof. Charles Joyce, Cambridge, Mass. (29).
White, Miss Florence, Pittsfield, Mass. (29).
White, Prof. Frances E., Woman's Med. College, Philadelphia, Pa. (29).
White, Prof. H. C., Univ. of Georgia, Athens, Ga. (29).

White, LeRoy S., Box 157, Waterbury, Conn. (23).
White, Thomas W., Hernando, De Soto Co., Miss. (28).
Whiteaves, J. F., Geological Survey, Ottawa, Can. (31).
Whitehouse, F. Cope, Brevoort House, New York, N. Y. (31).
Whiting, Henry L., U. S. C. and G. Survey, West Tisbury, Mass. (29).
Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31).
Whitman, Edmund B., 36 Trowbridge St., Cambridge, Mass. (29).
Whitman, Gilbert P., Manchester, N. H. (30).
Whitney, Mary W., Waltham, Mass. (19).
Whitney, Dr. Wm. F., Marlboro St., Boston, Mass. (29).
Wicksteed, R. J., LL. D., Ottawa, Province of Ont., Can. (29).
Wiechmann, F. G., 458 Broadway, New York, N. Y. (30).
Wilber, Francis A., Rutgers College, New Brunswick, N. J. (31).
Wilbour, Mrs. Charlotte B., care J. M. Furley, 74 W. 35th St., New York, N. Y. (28).
Wilcox, Mary A., Newnham College, Cambridge, Eng. (29).
Wilcox, W. A., Sec'y Boston Fish Bureau, 176 Atlantic Avenue, Boston, Mass. (29).
Wilder, Alex., M. D., 567 Orange St., Newark, N. J. (29).
Wilder, Graham, Louisville, Ky. (30).
Wilkinson, Mrs. L. V., Brazeau P. O., Perry Co., Mo. (30).
Willets, Joseph C., Skaneateles, N. Y. (29).
Williams, David, 83 Reade St., New York, N. Y. (29).
Williams, Mrs. Drue S., Greensburg, Decatur Co., Ind. (30).
Williams, Dr. E., 64 W. 7th St., Cincinnati, Ohio (30).
Williams, E. B., Strawberry Point, Iowa (24).
Williams, Prof. Edward H., jr., Box 463, Bethlehem, Pa. (25).
Williams, F. H., M. D., 100 Boylston St., Boston, Mass. (29).
Williams, Frank, Buffalo, N. Y. (25).
Williams, G. Henry, Baltimore, Md. (30).
Williams, J. Francis, Salem, N. Y. (31).
Williams, Janet W., Newton Lower Falls, Mass. (31).
Williams, P. O., M.D., Watertown, Jefferson Co., N. Y. (24).
Williams, Rev. R. R., Canajoharie, Montgomery Co., N. Y. (28).
Willoughby, Hugh L., Saratoga Springs, N. Y. (28).
Wills, William R., Waltham, Mass. (30).
Willson, Robert W., Box 853, New Haven, Conn. (30).
Wilmot, Thos. J., Direct U. S. Cable Co., Rye, N. H. (27).
Wilson, C. H., Rugby, Tenn. (30).
Wilson, H. C., Mt. Lookout, Hamilton Co., Ohio (30).
Wilson, M. C., Florence, Ala. (26).
Wilson, Prof. P. B., 44 Second St., Baltimore, Md. (30).
Wingate, Miss Hannah S., 208 Raymond St., Brooklyn, N. Y. (31).
Winslow, Sam'l W., Room 30, Rialto Building, 131 Devonshire, cor. Milk St., Boston, Mass. (29).
Winston, Prof. Chas. H., Richmond College, Richmond, Va. (30).

Witherbee, Frank S., care Witherbees, Sherman & Co., Port Henry, N. Y. (29).
Wolcott, Mrs. Henrietta L. T., Hotel Vendôme, Boston, Mass. (29).
WOOD, DR. ROBERT W., Jamaica Plain, Mass. (29).
Wood, Dr. Wm., Portland, Me. (29).
Woodbury, C. J. H., 131 Devonshire St., Boston, Mass. (29).
Woodward, J. J., M. D., Army Medical Museum, Washington. D. C. (28).
Woodward, Richard W., South Pueblo, Col. (29).
Woolman, Geo. S., 116 Fulton St., New York, N. Y. (29).
Worthen, T. W. D., Hanover, N. H. (28).
WRIGHT, ELIZUR, Box 109, Boston, Mass. (31).
Wright, Harrison, Sec'y Wyoming Hist. and Geol. Soc., Wilkes Barré, Pa. (29).
Wright, Smithson E., Treasurer's Office, L. M. R. R. Co., Cincinnati, Ohio (30).
Wylie, Samuel B., Bloomington, Ind. (30).

Youmans, Wm. Jay, M. D., Popular Science Monthly, 549 Broadway, New York, N. Y. (28).
Young, Prof. Andrew H., Hanover, Ind. (30).

Zimmerman, Charles D., Buffalo, N. Y. (30).
Zimmerman, William, Room 22, 142 Dearborn St., Chicago, Ill. (30).
Zinke, E. Gustav, M. D., Elm St., near 14th, Cincinnati, Ohio (30).
Zwick, G. A., 11th and Madison Sts., Covington, Ky. (30).

[1407 MEMBERS.]

NOTE.—The omission of an address in the foregoing list indicates that letters directed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

HONORARY FELLOW.[1]

*ROGERS, PROF. WILLIAM B., 117 Marlborough St., Boston, Mass. 1881.

FELLOWS.[2]

Abbe, Prof. Cleveland, Washington, D. C. (16). 1874.
Agassiz, Alex., Curator Mus. Comp. Zool., Cambridge, Mass. (18). 1875.
Alexander, John S., 1935 Arch St., Philadelphia, Pa. (20). 1874.
Allen, Dr. Harrison, 117 South 20th St., Philadelphia, Pa. (29). 1882.
Allen, Joel A., Mus. Comp. Zoology, Cambridge, Mass. (18). 1875.
Alvord, Benj., Brig. Gen., U. S. Army, 1207 Q St., N. W., Washington, D. C. (17). 1874.
Alvord, Henry E., C. E., Houghton Farm, Mountainville, Orange Co., N. Y. (29). 1882.
Anthony, Prof. Wm. A., Cornell Univ., Ithaca, N. Y. (28). 1880.
Appleton, Prof. John H., Brown Univ., Providence, R. I. (22). 1874.
Ashburner, Wm., 1014 Pine St., San Francisco, Cal. (29). 1882.
Atkinson, Edward, 131 Devonshire St., Boston, Mass. (29). 1881.
Atwater, Prof. W. O., Wesleyan Univ., Middletown, Conn. (29). 1882.
Austen, Peter T., Ph. D., Rutgers College, Lock Box No. 2, New Brunswick, N. J. (26). 1879.
Austin, E. P., 46 E. Newton St., Boston, Mass. (18). 1874.

Bailey, Prof. Loring W., University of Fredericton, N. B. (18). 1875.
Bailey, Prof. W. W., Brown University, Providence, R. I. (18). 1874.
Baird, Prof. S. F., Sec'y Smithsonian Inst., Washington, D. C. (1). 1875.
Baker, Marcus, Coast Survey Office, Washington, D. C. (30). 1882.
Ballou, Geo. F., Waltham, Mass. (29). 1881.
Bandelier, Ad. F., Highland, Ill. (25). 1879.
BARKER, PROF. G. F., Univ. of Penn., Philadelphia, Pa. (13). 1875.
Barnard, F. A. P., President Columbia College, New York, N. Y. (7). 1874.
Barnard, Prof. Wm. S., U. S. Dept. Agric., Washington, D. C. (24). 1880.
Bartlett, J. R., Comd'r U. S. N., care of C. & G. Survey Office, Washington, D. C. (30). 1882.
Bassett, Homer F., Waterbury, Conn. (23). 1874.
Batchelder, Dr. J. H., Salem, Mass. (18). 1874.
Batchelder, John M., 3 Divinity Avenue, Cambridge, Mass. (8). 1875.
Beal, Prof. Wm. James, Agricultural College, Lansing, Mich. (24). 1880.
Belknap, George E., Capt. U. S. N., care Fred. L. Wheeler & Co., Box 3522, Boston, Mass. (29). 1881.

[1] See ARTICLE VI of the Constitution. [2] See ARTICLE IV of the Constitution.

*** The number in parenthesis indicates the meeting at which the member joined the Association; the date at the end of the line the time when made a Fellow.

When the name is given in small capitals, it designates that the Fellow is also a life member, and is entitled to the Annual Volume of Proceedings.

* Deceased.

W. · ·. Alex. Graham, Scott Circle, 1500 Rhode Island Ave., Washing-
ı, D. C. (26). 1879.
Samuel N Manchester, N. H. (7). 1874.
Bessey, Prof. C. F., Agricultural College, Ames, Iowa (21). 1880.
Bethune, Rev. C. J. S., Trinity Coll. School, Pt. Hope, Canada (18). 1875.
Bickmore, Prof. Albert S., American Museum of Natural History, Central Park, New York, N. Y. (17). 1880. [1882.
Blair, Henry W., U. S. C. & G. Survey Office, Washington, D. C. (26).
Blake, Clarence J., M. D., Hotel Berkeley, Boston, Mass. (24). 1877.
Blake, Eli W., New Haven, Conn. (1). 1874.
Blake, Prof. Eli W., jr., Providence, R. I. (15). 1874.
Blake, Francis, Auburndale, Mass. (23). 1874.
Bolton, Dr. H. Carrington, Trinity Coll., Hartford, Conn. (17). 1875.
Borden, Spencer, Fall River, Mass. (29). 1882.
Boss, Prof. Lewis, Director Dudley Observ., Albany, N. Y. (26). 1878.
Bouvé, Thos. T., Boston Soc. Nat. Hist., Boston, Mass. (1). 1875.
Bowditch, Prof. H. P., Harvard Med. School, Boston, Mass. (28). 1880.
Bowditch, Henry I., M.D., 113 Boylston St., Boston, Mass. (2). 1875.
Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28). 1881.
Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19). 1875.
Breneman, A. A., Ithaca, N. Y. (20). 1880.
Brewer, Prof. Wm. H., New Haven, Conn. (20). 1875.
Britton, N. L., Columbia College, New York, N. Y. (29). 1882.
Broadhead, Garland Carr, Pleasant Hill, Cass Co., Mo. (27). 1879.
Brooks, Major Thomas B., Geol. State Surveys, Mich. and Wis., Newburgh, N. Y. (26). 1879.
Bross, Hon. Wm., Chicago, Ill. (7). 1874.
Broun, Prof. W. LeRoy, Vanderbilt Univ., Nashville, Tenn. (25). 1877.
Brown, Robert, jr., care of Yale College Observatory, New Haven, Conn. (11). 1874.
Brown, Mrs. Robert, jr., New Haven, Conn. (17). 1874.
Brush, Prof. George J., Yale College, New Haven, Conn. (4). 1874.
Buckhout, W. A., State College, Centre Co., Pa. (20). 1881.
Burgess, Edward, Sec. Natural History Soc., Boston, Mass. (22). 1877.
Burnham, S. W., 52 Vincennes Ave., Chicago, Ill. (25). 1877.
Burrill, Prof. T. J., Illinois Industrial Univ., Champaign, Ill. (29). 1882.

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875.
Call, Prof. R. Ellsworth, David City, Neb. (29). 1882.
Canby, Wm. M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878.
Capen, Rev. Francis L., Boston, Mass. (23). 1874.
Carhart, Prof. Henry S., Evanston, Ill. (29). 1881.
Carmichael, Prof. Henry, Bowdoin College, Brunswick, Me. (21). 1875.
Carpenter, Lieut. W. L., U. S. A., Dunkirk, N. Y. (24). 1877.
Carr, Lucien, Peabody Mus. Arch. and Ethn., Cambridge, Mass. (25). 1877.
Chamberlain, Wm. I., Sec. Ohio State Bd. of Agric., Columbus, Ohio (30). 1882.

Chamberlin, T. C., Beloit College, Beloit, Wis. (21). 1877.
Chandler, Prof. C. F., School of Mines, Columbia Coll., 50th St. cor. 4th Ave., New York, N. Y. (19). 1875. [1882.
Chandler, Seth C., jr., Harvard Observatory, Cambridge, Mass. (29).
Chandler, Prof. W. H., The Lehigh Univ., Bethlehem, Pa. (19). 1874.
Chanute, O., care Erie Railway, New York, N. Y. (17). 1877.
Chesbrough, E. S., Chicago, Ill. (2). 1877.
Chester, Prof. Albert H., Hamilton College, Clinton, N. Y. (29). 1882.
Chickering, Prof. J. W., jr., Deaf Mute College, Washington, D. C. (22). 1877.
Chittenden, Russell H., Ph. D., 95 Humphrey St., New Haven, Conn. (29). 1882.
Clark, Alvan G., Cambridgeport, Mass. (28). 1880. [1875.
Clark, Prof. John E., Mathematics, Yale College, New Haven, Conn. (17).
Clarke, Prof. F. W., U. S. Geological Survey, Washington, D. C. (18). 1874.
Claypole, Edw. W., B. A., New Bloomfield, Perry Co., Pa. (30). 1882.
Coffin, Prof. John H. C., U. S. Navy, Washington, D. C. (1). 1874.
Coffin, Prof. Selden J., Ph.D., Lafayette College, Easton, Pa. (22). 1874.
Collett, Prof. John, Chief Indiana Bureau of Statistics of Geology, Indianapolis, Ind. (17). 1874.
Collier, Peter, Dept. of Agric., Washington, D. C. (29). 1882.
Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Albany, N. Y. (28). 1880.
Comstock, J. Henry, Cornell Univ., Ithaca, N. Y. (28). 1882.
Comstock, Prof. M. L., Ph. D., Galesburg, Knox Co., Ill. (21). 1874.
Cook, Prof. A. J., Agricultural College, Lansing, Mich. (24). 1880.
Cook, Prof. George H., New Brunswick, N. J. (4). 1875.
Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19). 1880.
Cope, Prof. Edward D., 2100 Pine St., Philadelphia, Pa. (17). 1875.
Cox, Prof. Edward T. (19). 1874.
Cox, Hon. Jacob D., 103 Broadway, Cincinnati, Ohio (30). 1881.
Coxe, Eckley B., Drifton, Luzerne Co., Pa. (23). 1879.
Crocker, Susan E., M.D., Lawrence, Mass. (21). 1874.
Crosby, Wm. O., Mass. Inst. Technology, Boston, Mass. (29). 1881.
Cross, Prof. Chas. R., Mass. Inst. Technology, Boston, Mass. (29). 1880.
Cummings, Rev. Joseph, D.D., Pres. Northwestern Univ., Evanston, Ill. (13). 1874.
Curtis, Dr. Josiah, Washington, D. C. (18). 1874.
Cutting, Hiram A., M.D., State Geologist, Lunenburgh, Vt. (17). 1874.

Dabney, Chas. W., jr., Ph. D., Agric. Experiment Station, Raleigh, N. C. (30). 1882.
Dall, Mrs. Caroline H., 31st St., Washington, D. C. (18). 1874.
Dall, Wm. H., Smithsonian Institution, Washington, D. C. (18). 1874.
Dana, Edward Salisbury, New Haven, Conn. (23). 1875.
Dana, Prof. James D., New Haven, Conn. (1). 1875.

Danforth, Edward, Dep't of Public Instruction, Elmira, N. Y. (11). 1874.
Davidson, Prof. Geo., Asst. Coast and Geodetic Survey, San Francisco, Cal. (29). 1881.
Dawson, Dr. J. W., Principal McGill Coll., Montreal, Canada (10). 1875.
Day, F. H., M. D., Wauwatosa, Wis. (20). 1874.
Dean, George W., U. S. Coast Survey, Fall River, Mass. (15). 1874.
Dimmock, George, Cambridge, Mass. (22). 1874.
Dinwiddie, Robert, 117 W. 43d St., New York, N. Y. (1). 1874.
Dodge, Charles R., Washington, D. C. (22). 1874.
Dolbear, A. Emerson, College Hill, Mass. (20). 1880.
Draper, Dan'l, Ph.D., Director N. Y. Meteorological Observatory, Central Park, 64th St., Fifth Avenue, New York, N. Y. (29). 1881.
Drown, Prof. Thos. M., Lafayette College, Easton, Pa. (29). 1881.
Du Bois, Prof. Aug. J., New Haven, Conn. (30). 1882.
Dudley, Charles B., Altoona, Pa. (23). 1882.
Dudley, Wm. L., Miami Med. College, Cincinnati, Ohio (28). 1881.
Dunnington, Prof. F. P., University of Virginia, Va. (26). 1880.
Dutton, Capt. C. E., U. S. Ord. Dep't, Washington, D. C. (23). 1875.
Dwight, Prof. Wm. B., Vassar College, Poughkeepsie, N. Y. (30). 1882.
Dwyer, John, M. D., Hartford, Conn. (23). 1874.

Eads, Jas. B., 314 Chestnut Street, St. Louis, Mo. (27). 1879.
Eastman, Prof. J. R., U. S. Naval Observatory, Washington, D. C. (26). 1879.
Eaton, Prof. D. G., Packer Institute, Brooklyn, N. Y. (19). 1874.
Eddy, Prof. H. T., University of Cincinnati, Cincinnati, Ohio (24). 1875.
Edison, Thos. A., Menlo Park, N. J. (27). 1878.
Edmands, J. Rayner, Observatory, Cambridge, Mass. (29). 1880.
Egleston, Prof. Thos., School of Mines, Columbia College, New York, N. Y. (27). 1879.
Eimbeck, Wm., U.S.C.S., Washington, D. C. (17). 1874.
Elliott, Arthur H., School of Mines, Columbia College, New York, N. Y. (23). 1880.
Elliott, Ezekiel B., Treasury Dept., Washington, D. C. (10). 1874.
Elsberg, Louis, M.D., 614 Fifth Avenue, New York, N. Y. (23). 1874.
Emerson, Prof. Benjamin K., Amherst, Mass, (19). 1877.
Emerson, Prof. C. F., Dartmouth College, Hanover, N. H. (22). 1874.
Emerton, James H., Salem, Mass. (18). 1875.
Emmons, S. F., Box 2836, Denver, Col. (26). 1879.
Engelmann, Dr. George, St. Louis, Mo. (1). 1875.
Engelmann, Geo. J., M. D., 3003 Locust St., St. Louis, Mo. (25). 1878.
Ernst, Carl W., Daily Advertiser, Boston, Mass. (23). 1874.
Eustis, Prof. Henry L., Cambridge, Mass. (2). 1874.
Evans, Asher B., Principal Union School, Lockport, N. Y. (19). 1874.

Fairbanks, Henry, Ph. D., St. Johnsbury, Vt. (14). 1874.
Farlow, Dr. W. G., 6 Park Square, Boston, Mass. (20). 1875.

Farmer, Moses G., Torpedo Station, Newport, R. I. (9). 1875.
Farquharson, Dr. Robert James, Des Moines, Iowa (24). 1880.
Ferguson, Maj. Thos. B., Ass't U. S. Fish Commissioner, Washington, D. C. (28). 1881.
Fernald, Prof. Charles H., State Agricultural College, Orono, Me. (22). 1881.
Ferrel, Wm., U. S. Coast Survey Office, Washington, D. C. (11). 1875.
Ficklin, Prof. Joseph, Univ. of Missouri, Columbia, Mo. (20). 1878.
Fitch, Edward H., Jefferson, Ashtabula Co., Ohio (11). 1874.
Fletcher, Dr. Robert, Surgeon General's Office, Washington, D. C. (29). 1881.
Flint, James M., Surgeon U. S. N., 42 Dartmouth St., Boston, Mass. (28). 1882.
Foote, Dr A. E., 1223 Belmont Avenue, Philadelphia, Pa. (21). 1874.
Forbes, Prof. S. A., Normal, Ill. (27). 1879.
Frazer, Dr. Persifor, Ass't Geologist, 2d Geol. Survey of Pa., 917 Clinton St., Philadelphia, Pa. (24). 1879.
Frazier, Prof. B. W., The Lehigh University, Bethlehem, Pa. (24). 1882.
Freeman, H. C., Alto Pass, Union Co., Ill. (17). 1876.
Frisby, Prof. Edgar, U. S. N. Observ., Washington, D. C. (28). 1880.
Fuller, Andrew S., Ridgewood, Bergen Co., N. J. (24). 1882.

Gage, Simon Henry, Ithaca, N. Y. (28). 1881.
Gardiner, Rev. Frederic, D.D., Middletown, Conn. (23). 1874.
Gardner, James T., Director N. Y. State Survey, Albany, N. Y. (25). 1879.
Garland, Rev. Dr. L. C., Chancellor Vanderbilt Univ., Nashville, Tenn. (25). 1877.
Garman, S., Museum Comp. Zoology, Cambridge, Mass. (20). 1874.
Gatschet, Albert S., Bureau of Ethnology, Washington, D. C. (30). 1882.
Genth, Prof. F. A., Univ. of Pennsylvania, Philadelphia, Pa. (24). 1875.
Gilbert, G. K., Box 240, Salt Lake City, Utah (18). 1874.
Gillman, Henry, 155 Adams Avenue East, Detroit, Mich. (24). 1875.
Gilman, Daniel C., President of the Johns Hopkins University, Baltimore, Md. (10). 1875.
Goessman, Prof. C. A., Mass. Agric. Coll., Amherst, Mass. (18). 1875.
Goldschmidt, S. A., Ph. D., Morse Building, 140 Nassau St., New York, N. Y. (24). 1880.
Gooch, Frank A., U. S. Geological Survey, Newport, R. I. (25). 1880.
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875.
Goode, Prof. G. Brown, Curator National Museum, Washington, D. C. (22). 1874.
Goodfellow, Edward, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (24). 1879.
Grant, Mrs. Mary J., Brookfield, Conn. (23). 1874.
Gray, Prof. Asa, Botanic Gardens, Cambridge, Mass. (1). 1875.
Green, Traill, M. D., Easton, Pa. (1). 1874.
Grimes, J. Stanley (17). 1874.

Grote, Aug. R., New Brighton, Staten Island, N. Y. (22). 1875.
Guyot, Prof. Arnold, Princeton, N. J. (1). 1875.

Hagen, Dr. Hermann A., Mus. Comp. Zool., Cambridge, Mass. (17). 1875.
Hague, Arnold, 10 West 29th St., New York (26). 1879.
Hall, Prof. Asaph, U. S. Naval Observ., Washington, D. C. (25). 1877.
Hall, Edwin H., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Hall, Prof. James, Albany, N. Y. (1). 1875.
Hale, Horatio, Clinton, Ontario, Can. (30). 1882.
Hamlin, Dr. A. C., Bangor, Me. (10). 1874.
Harger, Oscar, Yale College Museum, New Haven, Conn. (25). 1879.
Harkness, Prof. Wm., U. S. N., U. S. Observatory, Washington, D. C. (26). 1878.
Harrington, Prof. Mark W., Ann Arbor, Mich. (22). 1875.
Harrison, Dr. B. F., Wallingford, Conn. (11). 1874.
Hasbrouck, Prof. I. E., Rutgers Coll., New Brunswick, N. J. (23). 1874.
Haskins, Rev. David Greene, Cambridge, Mass. (25). 1874.
Hastings, C. S., Johns Hopkins University, Baltimore, Md. (25). 1878.
Hazen, Brig. Gen. Wm. B., Chief Signal Office, U. S. A., Washington, D. C. (30). 1882.
Hedrick, B. S., Exam'r U. S. Pat. Office, Washington, D. C. (19). 1874.
Henderson, John G., Winchester, Ill. (25). 1879.
Henshaw, Henry W., Care Major J. W. Powell, 910 M St., Washington, D. C. (24). 1877.
Hervey, Rev. A. B., Taunton, Mass. (22). 1879.
Hilgard, Prof. E. W., Univ. of California, Berkeley, Cal. (11). 1874.
Hilgard, Prof. J. E., U. S. Coast Survey, Washington, D. C. (4). 1874.
Hill, Franklin C., P. O. Box 338, Princeton, N. J. (29). 1881.
Hill, George W., Nautical Almanac Office, Navy Department, Washington, D. C. (23). 1874.
Hill, Rev. Dr. Thomas, 738 Congress St., Portland, Me. (3). 1875.
Hill, Walter N., Chester, Pa. (29). 1881.
Himes, Prof. Charles F., Carlisle, Pa. (29). 1882.
Hitchcock, Prof. Charles H., Hanover, N. H. (11). 1874.
Hitchcock, Romyn, 53 Maiden Lane, New York, N. Y. (29). 1881.
Hodges, N. D. C., Salem, Mass. (29). 1882.
Hoffmann, Dr. Fred., 797 Sixth Ave., New York, N. Y. (28). 1881.
Holden, Prof. E. S., Director Washburn Observatory, Univ. of Wisconsin, Madison, Wis. (23). 1875.
Holmes, Dr. Oliver Wendell, 296 Beacon St., Boston, Mass. (29). 1881.
Horsford, Prof. E. N., Cambridge, Mass. (1). 1876.
Hough, Franklin B., Lowville, N. Y. (4). 1874.
Hoy, Philo R., M.D., Racine, Wis. (17). 1875.
Hunt, George, Providence, R. I. (9). 1874.
Hunt, Dr. T. Sterry, Montreal, Canada (1). 1874.
Huntington, Prof. J. H., Boston, Mass. (19). 1874.
Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18). 1875.

Hyatt, James, Stanfordville, Dutchess Co., N. Y. (10). 1874.
Hyde, Prof. E. W., Walnut Hills, Cincinnati, Ohio (25). 1881.

James, Jos. F., 177 Race St., Cincinnati, Ohio (30). 1882.
Jeffries, B. Joy, M. D., 15 Chestnut St., Boston, Mass. (29). 1881.
Jenks, Elisha T., Middleborough, Mass. (22). 1874.
Jenks, Prof. J. W. P., Middleborough, Mass. (2). 1874.
Jewell, Theo. F., Lt. Comdr. U.S.N., Torpedo Station, Newport, R. I. (25). 1882.
Jillson, Dr. B. C., Illinois Industrial Univ., Champaign, Ill. (14). 1881.
Johnson, Prof. S. W., Sheffield Sci. School, New Haven, Conn. (22). 1874.
Johnson, Prof. W. W., Annapolis, Md. (29). 1881.
Joy, Prof. Charles A., Stockbridge, Mass. (8). 1879.
Julien, A. A., School of Mines, Columbia Coll., New York, N. Y. (24). 1875.

Kedzie, Prof. Robert C., Lansing, Mich. (29). 1881.
Kendall, Prof. E. Otis, 3826 Locust St., Philadelphia, Pa. (29). 1882.
Kennan, George, 1318 Mass. Avenue, Washington, D. C. (30). 1882.
Kent, William, Pittsburgh, Pa. (26). 1881.
Kerr, Prof. W. C., Raleigh, N. C. (10). 1874.
Kirkwood, Prof. Daniel, Bloomington, Ind. (7). 1874.

Lambert, Rev. Thomas R., D.D., Charlestown, Mass. (18). 1874.
Langdon, Dr. F. W., 65 West 7th St., Cincinnati, Ohio (30). 1882.
Langley, Prof. J. W., Univ. of Michigan, Ann Arbor, Mich. (23). 1875.
Langley, Prof. S. P., Director of Observatory, Allegheny, Pa. (18). 1874.
Lanza, Prof. Gaetano, Mass. Inst. of Technology, Boston, Mass. (29). 1882.
Lattimore, Prof. S. A., Univ. of Rochester, Rochester, N. Y. (15). 1874.
Lawrence, George N., 45 E 21st St., New York, N. Y. (7). 1877.
Lazenby, Prof. Wm. R., Ithaca, N. Y. (30). 1882.
Lea, Dr. Isaac, 1622 Locust St., Philadelphia, Pa. (1). 1875.
LeConte, Dr. John L., 1625 Spruce St., Philadelphia, Pa. (1). 1874.
LeConte, Prof. Joseph, Univ. of Cal., Berkeley, Cal. (29). 1881.
Ledoux, Albert R., Ph. D., 17 Cedar St., New York, N. Y. (26). 1881.
Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23). 1874.
Lesley, Prof. J. Peter, State Geol. of Pa., 1008 Clinton St., Philadelphia, Pa. (2). 1874.
Lewis, Prof. Henry C., E. Washington Lane, Germantown, Pa. (26). 1880.
Lilly, Gen. Wm., Mauch Chunk, Carbon Co., Pa. (28). 1882.
Lindsley, Dr. J. Berrien, Nashville, Tenn. (1). 1874.
Lintner, J. A., N. Y. State Mus. of Nat. Hist., Albany, N. Y. (22). 1874.
Little, W. C., Albany, N. Y. (22). 1874.
Litton, Abram, Washington University, St. Louis, Mo. (28). 1879.
Lockwood, Rev. Samuel, Freehold, Monmouth Co., N. J. (18). 1875.
Loomis, Prof. Elias, New Haven, Conn. (1). 1874.

Lord, Prof. Nat. W., State Univ., Columbus, Ohio (29). 1881.
Loudon, Prof. James, Toronto, Can. (25). 1881.
Loughridge, Prof. R. H., Berkeley, Cal. (21). 1874.
Love, Edward G., School of Mines, Columbia College, New York, N. Y. (24). 1882.
Lovering, Prof. Joseph, Harvard University, Cambridge, Mass. (2). 1875.
Lull, Edward P., Captain U. S. N., Navy Yard, Boston, Mass. (28). 1880.
Lupton, Prof. N. T., Vanderbilt University, Nashville, Tenn. (17). 1874.
Lyle, David Alexander, Capt. of Ordnance U. S. A., National Armory, Springfield, Mass. (28). 1880.
Lyman, Prof. Chester S., New Haven, Conn. (4). 1875.
Lyman, Hon. Theodore, Brookline, Mass. (23). 1875.
Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

Mabery, C. F., Cambridge, Mass. (29). 1881.
Macfarlane, James, Towanda, Pa. (29). 1882.
Macloskie, Prof. George, College of New Jersey, Princeton, N. J. (25). 1882.
Mallery, Brevet Lieut. Col. Garrick, U. S. Army, Box 585, Washington, D. C. (26). 1879.
Mann, B. Pickman, U. S. Dep't of Agric., Washington, D. C. (22). 1874.
Marcy, Oliver, LL.D., Evanston, Ill. (10). 1874.
Mark, Edw. L., 48 Shepard St., Cambridge, Mass. (21). 1879.
Markoe, Geo. F. H., 61 Warren St., Roxbury, Mass. (29). 1881.
Marsh, Prof. O. C., Yale College, New Haven, Conn. (15). 1874.
Martin, Prof. Daniel S., Rutgers Female College, New York, N. Y. (23). 1879.
Martin, Prof. H. Newell, Johns Hopkins Univ., Baltimore, Md. (27). 1880.
Mason, Prof. Otis T., Columbian University, Washington, D. C. (25). 1877.
Mayer, Prof. A. M., So. Orange, N. J. (19). 1874.
McCauley, Capt. C. A. H., Ass't Q. M., U. S. A., 321 Michigan Ave., Chicago, Ill. (29). 1881.
McGee, W. J., U. S. Geol. Survey, Salt Lake City, Utah (27). 1882.
McMurtrie, William, Dep't Agriculture, Washington, D. C. (22). 1874.
McNair, Lt. Commander Antoine R., U. S. N., Box 1071, Saratoga Springs, N. Y. (28). 1882.
McRae, Hamilton S., Supt. of Schools, Muncie, Ind. (20). 1874.
Meehan, Thomas, Germantown, Pa. (17). 1875.
Mees, Carl Leo, Louisville, Ky. (24). 1876.
Mendenhall, Prof. T. C., Columbus, Ohio (20). 1874.
Merriman, C. C., Rochester, N. Y. (29). 1880.
Michelson, A. A., Master U. S. N., Nautical Almanac Office, Washington, D. C. (26). 1879.
Minot, Charles Sedgwick, Roslindale P. O., Boston, Mass. (28). 1880.
Mitchell, Miss Maria, Vassar College, Poughkeepsie, N. Y. (4). 1874.
Mixter, Prof. Wm. G., New Haven, Conn. (30). 1882.

Moore, Prof. J. W., M. D., Lafayette College, Easton, Pa. (22). 1874.
Morley, Prof. Edward W., Hudson, Ohio (18). 1876.
Morris, Rev. John G., Baltimore, Md. (12). 1874.
Morse, Prof. E. S., Salem, Mass. (18). 1874.
Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1875.
Munroe, Prof. C. E., U. S. Naval Academy, Annapolis, Md. (22). 1874.
Murtfeldt, Miss Mary E., Kirkwood, Mo. (27). 1881.

Nason, Prof. H. B., Rensselaer Polytechnic Inst., Troy, N. Y. (13). 1874.
Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882.
Nelson, Prof. Edward T., Delaware, Delaware Co., Ohio (24). 1877.
Newberry, Prof. J. S., Cleveland, Ohio, and Columbia College, New York, N. Y. (5). 1875.
Newcomb, Prof. S., U. S. Naval Observ., Washington, D. C. (13). 1874.
Newton, Hubert A., New Haven, Conn. (6). 1874.
Nichols, E. L., Ph. D., Central Univ. Richmond, Ky. (28). 1881.
Nichols, Prof. W. R., Mass. Inst. Technology, Boston, Mass. (18). 1875.
Niles, Prof. W. H., Cambridge, Mass. (16). 1874.
Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24). 1876.
Norton, Prof. W. A., New Haven, Conn. (6). 1875.
Norwood, Charles J., Ass't Kentucky Geological Survey, Russellville, Ky. (26). 1881.

Oliver, Prof. James E., Cornell University, Ithaca, N. Y. (7). 1875.
Oliver, Miss Mary E., Cascadilla Hotel, Ithaca, N. Y. (20). 1874.
Ordway, Prof. John M., Mass. Inst. Technology, Boston, Mass. (9). 1875.
Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875.
Osborne, J. W., 212 Delaware Ave. N. E., Washington, D. C. (22). 1874.
Osler, Prof. William, McGill College, Montreal, Canada (28). 1881.
Owen, Dr. Richard, New Harmony, Ind. (20). 1874.

Packard, Dr. A. S., jr., 115 Angell St., Providence, R. I. (16). 1875.
Paine, Cyrus F., Rochester, N. Y. (12). 1874.
Paine, Nathaniel, Worcester, Mass. (18). 1874.
Palfray, Hon. Charles W., Salem, Mass. (21). 1874.
Parke, John G., Lt. Col. Corps of Eng'rs, Bv't Maj. Gen. U. S. A., Office of Chief of Engineers, Washington, D. C. (29). 1881.
Parkhurst, H. M., Law Stenographer, 25 Chambers St., New York, N. Y. (23). 1874.
Peckham, S. F., Providence, R. I. (18). 1875.
Pedrick, Wm. R., Lawrence, Mass. (22). 1875.
Peet, Rev. Stephen D., Clinton, Wis. (24). 1881.
Peirce, B. O., Beverly, Mass. (18). 1874.
Penhallow, Prof. D. P., Experiment Station, Mountainville, N. Y. (30). 1882.

Perkins, Prof. George H., Burlington, Vt. (17). 1882.
Perkins, Maurice, Schenectady, N. Y. (15). 1875.
Peter, Dr. Robert, Ky. Geol. Survey, Lexington, Ky. (29). 1881.
Pettee, Prof. William H., Ann Arbor, Mich. (24). 1875.
Phillips, A. W., New Haven, Conn. (24). 1879.
Phippen, Geo. D., Salem, Mass. (18). 1874.
Pickering, Prof. E. C., Director of Observ., Cambridge, Mass. (18). 1875.
Pike, Dr. William H., University College, Toronto, Can. (29). 1881.
Pilling, James C., Box 585, Washington, D. C. (28). 1882.
Platt, Franklin, Ass't Geologist, 2nd Geol. Survey of Pa., 615 Walnut St., Philadelphia, Pa. (27). 1882.
Potter, Prof. William B., Washington University, St. Louis, Mo. (25). 1879.
Powell, Major J. W., U. S. Geologist, 910 M St., Washington, D. C. (23). 1875.
Prentiss, D. Webster, M. D., Washington, D. C. (29). 1882.
Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875.
Prime, Frederick, jr., Allentown Iron Co., 230 So. 3d St., Philadelphia, Pa. (24). 1875.
Pritchett, Henry S., Observatory, Washington Univ., St. Louis, Mo. (29). 1881.
Procter, John R., Director Ky. Geol. Survey, Frankfort, Ky. (26). 1881.
Pulsifer, Wm. H., St. Louis, Mo. (26). 1879.
Pumpelly, Prof. Raphael, U. S. Geol. Survey, Newport, R. I. (17). 1875.
Putnam, F. W., Curator Peabody Museum Archæology and Ethnology, Cambridge, Mass. Address as Permanent Secretary A. A. A. S., Salem, Mass. (10). 1874.
Putnam, James J., M. D., 63 Marlborough St., Boston, Mass. (28). 1880.
Pynchon, Rev. T. R., Pres. Trinity College, Hartford, Conn. (23). 1875.

Quimby, Prof. E. T., Hanover, N. H. (22). 1874.
Quincy, Edmund, 88 Clinton St., Boston, Mass. (11). 1874.

Rauch, Dr. John H., Chicago, Ill. (11). 1875.
Raymond, Rossiter W., 17 Burling Slip, New York, N. Y. (15). 1875.
Redfield, J. H., care A. Whitney & Sons, Philadelphia, Pa. (1). 1874.
Reed, E. Baynes, London, Ontario, Can. (27). 1882.
Rees, Prof. John K., Washington University, St. Louis, Mo. (26). 1878.
Remsen, Prof. Ira, Johns Hopkins University, Baltimore, Md. (22). 1875.
Rice, John M., U. S. Naval Academy, Annapolis, Md. (25). 1881.
Rice, Prof. Wm. North, Middletown, Conn. (18). 1874.
Richards, Prof. Robert H., Mass. Inst. Tech., Boston, Mass. (22). 1875.
Richards, Mrs. Robert H., Mass. Inst. of Tech., Boston, Mass. (23). 1878.
Ricketts, Pierre de Peyster, Ph.D., 10 Cedar St., New York, N. Y. (26). 1880.

RILEY, PROF. C. V., U. S. Entomologist, 1700 13th St. N.W., Washington, D. C. (17). 1874.
Ritchie, E. S., 150 Tremont St., Boston, Mass. (10). 1877.
Rockwell, Gen. Alfred P., Office Great Falls Manufacturing Co., Boston, Mass. (10). 1882.
Rockwood, Prof. Charles G., jr., Princeton, N. J. (20). 1874.
Rogers, Fairman, 202 West Rittenhouse Square, Philadelphia, Pa. (11). 1874.
Rogers, Prof. Robert E., Jefferson Medical College, Philadelphia, Pa. (18). 1874.
Rogers, W. A., Ass't Prof. Harvard College Observ., Cambridge, Mass. (15). 1875.
Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879.
Rood, Prof. O. N., Cor. E. 49th St. & Madison Ave., New York, N. Y. (14). 1875.
Roosevelt, Clinton, 11 Wall St., New York, N. Y. (11). 1874.
Ross, Waldo O., 189 Devonshire St., Boston, Mass. (29). 1882.
Rowland, Prof. Henry A., Baltimore, Md. (29). 1880.
Runkle, Prof. J. D., Mass. Inst. of Tech., Boston, Mass. (2). 1875.
Russell, I. C., U. S. Geological Survey, Salt Lake City, Utah (25). 1882.
Rutherford, Lewis M., 175 Second Ave., New York, N. Y. (13). 1875.

Safford, Dr. James M., Nashville, Tenn. (6). 1875.
Sampson, Comdr. W. T., U. S. N., Naval Observatory, Washington, D. C. (25). 1881.
Saunders, William, London, Ontario, Canada (17). 1874.
Schanck, Prof. J. Stillwell, Princeton, N. J. (4). 1882.
Schott, Charles A., U. S. Coast and Geodetic Survey Office, Washington, D. C. (8). 1874.
Schweitzer, Prof. Paul, State Univ. of Mo., Columbia, Mo. (24). 1877.
SCUDDER, SAMUEL H., Cambridge, Mass. (13). 1874.
Seaman, W. H., Microscopist, 1424 11th St. N. W., Washington, D. C. (23). 1874.
Seiler, Carl, M. D., Philadelphia, Pa. (29). 1882.
Sestini, Rev. B., Woodstock, Md. (29). 1882.
Sheafer, P. W., Pottsville, Pa. (4). 1879.
Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.
Sigsbee, Chas. D., Comd'r U. S. N., Navy Dep't, Washington, D. C. (28). 1882.
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio (6). 1874.
Silliman, Prof. Benj., Yale College, New Haven, Conn. (1). 1874.
Silliman, Prof. Justus M., Lafayette College, Easton, Pa. (19). 1874.
Skinner, Joseph J., New Britain, Conn. (23). 1880.
Smith, Prof. E. A., University of Alabama, Tuscaloosa, Ala. (20). 1877.
Smith, Mrs. Erminnie A., 203 Pacific Ave., Jersey City, N. J. (25). 1880.
Smith, Prof. F. H., University of Virginia, Charlottesville, Va. (26). 1880.
Smith, Dr. J. Lawrence, Louisville, Ky. (1). 1874.
SMITH, QUINTIUS C., M. D., Box 707, Austin, Texas (26). 1881.

Smith, Prof. S. I., Yale College, New Haven, Conn. (18). 1875.
Smock, Prof. John Conover, Box 282, New Brunswick, N. J. (23). 1879.
Snow, Prof. F. H., Lawrence, Kan. (29). 1881.
Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa. (24). 1882.
Spencer, Prof. J. William, Univ. of Missouri, Columbia, Mo. (28). 1882.
Spencer, John W., Geol., Paxton, Sullivan Co., Ind. (20). 1874.
Springer, Dr. Alfred, Box 573, Cincinnati, Ohio (24). 1880.
Stallo, J. B., Masonic Temple, Cincinnati, Ohio (30). 1882.
Stanard, Benjamin A., Cleveland, Ohio (6). 1874.
Stearns, Henry P., M.D., Hartford, Conn. (23). 1874.
Stearns, R. E. C., Box 13, Berkeley, Cal. (18). 1874.
Steiner, Dr. Lewis H., Frederick City, Md. (7). 1874.
Stephens, W. Hudson, Lowville, N. Y. (18). 1874.
Sternberg, George M., Surgeon U.S.A., Care Surgeon General, U.S.A., Washington, D. C. (24). 1880.
Stevens, W. LeConte, 170 Joralemon St., Brooklyn, N. Y. (29). 1882.
Stockwell, John N., 579 Case Avenue, Cleveland, Ohio (18). 1875.
Stone, George H., Colorado Springs, Col. (29). 1882.
Stone, Mrs. Leander, 3352 Indiana Avenue, Chicago, Ill. (22). 1874.
Stone, Ormond, Director Leander McCormick Observatory, Univ. of Va., Va. (24). 1876.
Storrs, Henry E., Jacksonville, Ill. (20). 1874.
Story, Wm. E., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874.
Sturtevant, E. Lewis, M. D., Geneva, N. Y. (29). 1882.
Sutton, Dr. George, Aurora, Dearborn Co., Ind. (20). 1881.
Swallow, Prof. G. C., Columbia, Mo. (10). 1875.
Swift, Lewis, Ph. D., Rochester, N. Y. (29). 1882.

Tainter, Sumner, 1221 Connecticut Ave., Washington, D. C. (29). 1881.
Tappan, Prof. Eli T., Kenyon College, Gambier, Ohio (20). 1874.
Taylor, Arthur F., Ph. D., Case School of Applied Science, Cleveland, Ohio (29). 1882.
Taylor, Wm. B., Smithsonian Institution, Washington, D. C. (29). 1881.
Terry, Prof. N. M., U. S. Naval Academy, Annapolis, Md. (23). 1874.
Thomas, Benj. F., Ph. D., Univ. State of Missouri, Columbia, Mo. (29). 1882.
Thurston, Prof. R. H., Stevens Institute, Hoboken, N. J. (23). 1875.
Todd, Prof. David P., Director Lawrence Observatory, Amherst College, Amherst, Mass. (27). 1881.
Townshend, Prof. N. S., Columbus, Ohio (17). 1881.
Tracy, Sam'l M., Columbia, Boone Co., Mo. (27). 1881.
Trembley, J. B., M.D., Oakland, Alameda Co., Cal. (17). 1880.
Trowbridge, Prof. John, Harvard University, Cambridge, Mass. (25). 1876.
Trowbridge, Prof. W. P., New Haven, Conn. (10). 1874.

True, Fred W., U. S. National Museum, Washington, D. C. (28). 1882.
Trumbull, Dr. J. Hammond, Hartford, Conn. (29). 1882.
Tuttle, Prof. Albert H., Columbus, Ohio (17). 1874.

Uhler, Philip R., Baltimore, Md. (19). 1874.
Upham, Warren, Nashua, N. H. (25). 1880.

Van der Weyde, P. H., M. D., Box 3619, New York, N. Y. (17). 1874.
Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28). 1882.
Van Vleck, Prof. John M., Middletown, Conn. (23). 1875.
Verrill, Prof. A. E., Yale College, New Haven, Conn. (16). 1875.
Vose, Prof. George L., Mass. Inst. Technology, Boston, Mass. (15). 1875.

Wadsworth, M. Edward, Ph. D., Assistant in Lithology, Museum of Comparative Zoology, Cambridge, Mass. (23). 1874.
Walcott, Charles D., U. S. Geol. Survey, Washington, D. C. (25). 1882.
Waldo, Leonard, S. D., New Haven, Conn. (28). 1880.
Walker, Prof. J. R., 154 Canal St., New Orleans, La. (19). 1874.
Walker, Prof. Joseph B. (20). 1874.
Wallace, Wm., Ansonia, Conn. (28). 1882.
Waller, E., School of Mines, Columbia College, New York, N. Y. (23). 1874.
Walling, H. F., U. S. Coast and Geodetic Survey Office, Washington, D. C. (16). 1874.
Ward, Prof. Henry A., Rochester, N. Y. (13). 1875.
Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879.
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874.
Warder, Prof. Robert B., North Bend, Hamilton Co., Ohio (19). 1881.
Warner, James D., 199 Baltic St., Brooklyn, N. Y. (18). 1874.
Warren, Cyrus M., Brookline, Mass. (29). 1882.
Warren, Prof. S. Edward, Newton, Mass. (17). 1875.
Watson, Sereno, Botanic Gardens, Cambridge, Mass. (22). 1875.
Wead, Prof. Charles K., Univ. of Michigan, Ann Arbor, Mich. (23). 1880.
Webster, Prof. H. E., Union College, Schenectady, N. Y. (23). 1882.
Webster, Prof. N. B., Principal Webster Inst., Norfolk, Va. (7). 1874.
Wells, Daniel H., Hartford, Conn. (18). 1875.
Westcott, O. S., Maywood, Ill. (21). 1874.
Wheatland, Dr. Henry, President Essex Inst., Salem, Mass. (1). 1874.
Wheeler, Capt. Geo. M., U. S. E., Lock Box 93, Washington, D. C. (25). 1879.
Wheeler, Orlando B., Ass't U. S. Lake Survey, Detroit, Mich. (24). 1882.
Wheildon, W. W., Concord, Mass. (13). 1874.
White, Prof. C. A., Le Droit Park, Washington, D. C. (17). 1875.
White, Prof. I. C., Univ. of W. Va., Morgantown, W. Va. (25). 1882.

Whitfield, R. P., American Museum Natural History, 77th St. & 8th Avenue, New York, N. Y. (18). 1874.
Whitman, Chas. O., care G. F. Leonard, Newton Highlands, Mass. (27). 1880.
Whitney, Solon F., Watertown, Mass. (20). 1874.
Whittlesey, Col. Charles, 1305 Euclid Ave., Cleveland, Ohio (1). 1875.
Wilber, G. M., Pine Plains, N. Y. (19). 1874.
Wilbur, A. B., Port Jervis, Orange Co., N. Y. (23). 1874.
Wilder, Prof. Burt G., Cornell University, Ithaca, N. Y. (22). 1875.
Wiley, Prof. Harvey W., Dept. of Agric., Washington, D. C. (21). 1874.
Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.
Williams, H. S., Cornell Univ., Ithaca, N. Y. (18). 1882.
Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11). 1874.
Wilson, Prof. Daniel, President University College, 117 Bloor St., Toronto, Canada (25). 1876.
Winchell, Prof. Alex., Ann Arbor, Mich. (3). 1875.
Winchell, Prof. N. H., Univ. of Minnesota, Minneapolis, Minn. (19). 1874.
Winsor, Justin, Librarian Harvard Univ., Cambridge, Mass. (29). 1882.
Woerd, Chas. V., Am. Watch Co., Waltham, Mass. (29). 1881.
Wood, Prof. De Volson, Hoboken, N. J. (29). 1881.
Wormley, T. G., Univ. of Pennsylvania, Philadelphia, Pa. (20). 1878.
Worthen, A. H., Springfield, Ill. (5). 1874.
Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880.
Wright, Prof. Arthur W., Yale College, New Haven, Conn. (14). 1874.
Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio (29). 1882.
Würtele, Rev. Louis C., Acton Vale, Province of Quebec, Can. (11). 1875.
Wyckoff, Wm. C., 44 Howard St., New York, N. Y. (20). 1874.
Wylie, Prof. Theoph. A., Indiana Univ., Bloomington, Ind. (20). 1874.

Youmans, Prof. Edward L., New York, N. Y. (6). 1874.
Young, C. A., Prof. of Astronomy, College of New Jersey, Princeton, N. J. (18). 1874.

Zentmayer, Joseph, 147 So. Fourth St., Philadelphia, Pa. (29). 1882.

[515 FELLOWS.]

TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 1922.

DECEASED MEMBERS.

[Information respecting omissions in this list, and the date of birth and of decease of any of the former members, is requested by the Permanent Secretary.]

Abbe, George W., New York, N. Y. (23). Died Sept. 25, 1879.

Abert, J. J., Washington, D. C. (1). Born in 1785. Died January 27, 1863.

Adams, C. B., Amherst, Mass. (1). Born January 11, 1814. Died Jan'y [19, 1853.

Adams, Edwin F., Charlestown, Mass. (18).

Adams, Samuel, Jacksonville, Ill. (18). Born Dec. 19, 1806. Died April 29, 1877.

Agassiz, Louis, Cambridge, Mass. (1). Born May 28, 1807. Died Dec. [14, 1873.

Ainsworth, J. G., Barry, Mass. (14).

Allen, Thomas, St. Louis, Mo. (27). Died April 8, 1882.

Allen, Zachariah, Providence, R. I. (1). Died March 17, 1882.

Allston, R. F. W., Georgetown, S. C. (3). Born April 21, 1801. Died April 7, 1864.

Ames, M. P., Springfield, Mass. (1). Born in 1803. Died April 23, 1847.

Andrews, Ebenezer B., Lancaster, Ohio (7). Died Aug. 21, 1880, aged 59.

Anthony, Charles H., Albany, N. Y. (6). Died in 1874.

Appleton, Nathan, Boston, Mass. (1). Born Oct. 6, 1779. Died July 14, 1861.

Armstrong, John W., Fredonia, N. Y. (24).

Atwater, Mrs. S. T., Chicago, Ill. (17). Born Aug. 8, 1812. Died April 11, 1878.

Aufrecht, Louis, Cincinnati, Ohio (30).

Bache, Alexander D., Washington, D. C. (1). Born July 19, 1806. Died Feb. 17, 1867.

Bache, Franklin, Philadelphia, Pa. (1). Born Oct. 25, 1792. Died March 19, 1864.

Bailey, Jacob W., West Point, N. Y. (1). Born April 29, 1811. Died Feb. 26, 1857.

Bardwell, F. W., Lawrence, Kan. (18). Died in 1878.

Barnard, John G., New York, N. Y. (14). Died May 14, 1882.

Barrett, Moses, Milwaukee, Wis. (21). Died in 1873.

Barry, Redmond, Melbourne, Australia (25).

Beck, C. F., Philadelphia, Pa. (1).

Beck, Lewis C., New Brunswick, N. J. (1). Born Oct. 4, 1798. Died April 21, 1853.

Beck, T. Romeyn, Albany, N. Y. (1). Born Aug. 11, 1791. Died Nov. 19, 1855.

Belfrage, G. W., Clifton, Texas (29). Died Dec. 7, 1882.
Belt, Thomas, London, Eng. (27). Died Sept. 8, 1878.
Benedict, George W., Burlington, Vt. (16). Born Jan. 11, 1796. Died in 1871.
Bicknell, Edwin, Boston, Mass. (18). Born in 1830. Died March 19,1877.
Binney, Amos, Boston, Mass. (1). Born Oct. 18, 1803. Died Feb. 18, [1847.
Binney, John, Boston, Mass. (3).
Blackie, Geo. S., Nashville, Tenn. (26).
Blake, Homer C., New York, N. Y. (28). Born Feb. 1, 1822.
Blanding, William, ———, R. I. (1).
Blatchford, Thomas W., Troy, N. Y. (6).
Blatchley, Miss S. L., New Haven, Conn. (19).
Boadle, John, Haddonfield, N. J. (20). Born in 1805. Died in July, 1878.
Bomford, George, Washington, D. C. (1). Born 1780. Died March 25, 1848.
Bowron, James, South Pittsburg, Tenn. (26). Died in Dec., 1877.
Bradley, Leverette, Jersey City, N. J. (15). Died in 1875.
Braithwaite, Jos., Chambly, C. W. (11).
Briggs, Albert D., Springfield, Mass. (13). Died Feb. 20, 1881.
Briggs, Robert, Philadelphia, Pa. (29).
Brigham, Charles H., Ann Arbor, Mich. (17). Born July 27, 1820. Died in Jan., 1879.
Brown, Andrew, Natchez, Miss. (1).
Burnap, G. W., Baltimore, Md. (12). Born Nov. 30, 1802. Died Sept. 8, 1859.
Burnett, Waldo I., Boston, Mass. (1). Died July 1, 1854, aged 27.
Butler, Thomas B., Norwalk, Conn. (10). Born Aug. 22, 1806. Died June 8, 1873.

Cairns, F. A., New York, N. Y. (27). Died in 1879.
Campbell, Mrs. Mary H., Crawfordsville, Ind. (22). Died Feb. 27, 1882.
Carpenter, Thornton, Camden, S. C. (7).
Carpenter, William M., New Orleans, La. (1).
Case, Leonard, Cleveland, Ohio (15). Born June 27, 1820. Died Jan. 5, 1880.
Case, William, Cleveland, Ohio (6).
Caswell, Alexis, Providence, R. I. (2). Born Jan. 29, 1799. Died Jan. 8, 1877.
Chadbourne, Paul Ansel, Amherst, Mass. (10). Born Oct. 21, 1823. Died Feb. 23, 1883.
Chapman, N., Philadelphia, Pa. (1). Born May 28, 1780. Died July 1, 1853.
Chase, Stephen, Hanover, N. H. (2). Born in 1813. Died Aug. 5, 1851.
Chauvenet, William, St. Louis, Mo. (1). Born May 24, 1819. Died Dec. 13, 1870.
Cheney, Miss Margaret S., Jamaica Plain, Mass. (29).
Clapp, Asahel, New Albany, Ind. (1). Born Oct. 5, 1792. Died Dec. 15, 1862.

Clark, Henry James, Cambridge, Mass. (13). Died July 1, 1873, aged 47.
Clark, Joseph, Cincinnati, Ohio (5).
Clarke, A. B., Holyoke, Mass. (13).
Cleaveland, C. H., Cincinnati, Ohio (9).
Cleveland, A. B., Cambridge, Mass. (2).
Coffin, James H., Easton, Pa. (1). Born Sept. 6, 1806. Died Feb. 6, 1873.
Cole, Thomas, Salem, Mass. (1). Born Dec. 24, 1779. Died June 24, 1852.
Coleman, Henry, Boston, Mass. (1).
Collins, Frederick, Washington, D. C. (28). Born Dec. 5, 1842. Died Oct. 27, 1881.
Conrad, Timothy Abbott, Philadelphia, Pa. (1). Born in August, 1803. Died August 9, 1877.
Cooke, Caleb, Salem, Mass. (18). Born Feb. 15, 1838. Died June 5, 1880.
Cooper, William, Hoboken, N. J. (9). Died in 1864.
Corning, Erastus, Albany, N. Y. (6). Born Dec. 14, 1794. Died April 9, 1872.
Couper, James Hamilton, Darien, Ga. (1). Born March 5, 1794. Died July 3, 1866.
Cramp, J. M., Wolfville, N. S. (11). Born July 25, 1796. Died Dec. 6, 1881.
Crocker, Charles F., Lawrence, Mass. (22). Died in July, 1881.
Crosby, Alpheus, Salem, Mass. (10). Born Oct. 13, 1810. Died Apr. 17, 1874.
Crosby, Thomas R., Hanover, N. H. (18). Born Oct. 22, 1816. Died March 1, 1872.
Croswell, Edwin, Albany, N. Y. (6). Born in May, 1797. Died June 13,
Curry, W. F., Geneva, N. Y. (11). [1871.

Dalrymple, E. A., Baltimore, Md. (11). Died Oct. 30, 1881.
Dayton, Edwin A., Madrid, N. Y. (7). Born in 1827. Died June 24, 1873.
Dean, Amos, Albany, N. Y. (6). Born Jan. 16, 1803. Died Jan. 26, 1868.
Dearborn, George H. A. S., Roxbury, Mass. (1).
Dekay, James E., New York, N. Y. (1). Born in 1792. Died Nov. 21, 1851.
DeLaski, John, Carver's Harbor, Me. (18).
Dewey, Chester, Rochester, N. Y. (1). Born Oct. 25, 1781. Died Dec. 15,
Dexter, G. M., Boston, Mass. (11). [1867.
Dillingham, W. A. P., Augusta, Me. (17).
Doggett, Wm. E., Chicago, Ill. (17). Born Nov. 20, 1820. Died in 1876.
Doolittle, L., Lenoxville, C. E. (11). Died in 1862.
Dorr, E. P., Buffalo, N. Y. (25). Died March 28, 1881.
Draper, Henry, New York, N. Y. (28).
Ducatel, J. T., Baltimore, Md. (1).
Duffield, George, Detroit, Mich. (10). Born July 4, 1794. Died June 26, 1869.
Dumont, A. H., Newport, R. I. (14).
Duncan, Lucius C., New Orleans, La. (10). Died Aug. 9, 1855, aged 54.
Dunn, R. P., Providence, R. I. (14).

Easton, Norman, Fall River, Mass. (14).
Eaton, James H., Beloit, Wis. (17). Died Jan. 5, 1877.
Ely, Charles Arthur, Elyria, Ohio (4).
Emerson, Geo. B., Boston, Mass. (1). Born Sept. 12, 1797. Died March 4, 1881.
Emmons, Ebenezer, Williamstown, Mass. (1). Born May 16, 1799. Died October 1, 1863.
Engstrom, A. B., Burlington, N. J. (1).
Everett, Edward, Boston, Mass. (2). Born April 11, 1794. Died Jan. 15, 1865.
Ewing, Thomas, Lancaster, Ohio (5). Born Dec. 28, 1789. Died Oct. 26, 1871.

Faries, R. J., Wauwatosa, Wis. (21). Died May 31, 1878.
Ferris, Isaac, New York, N. Y. (6). Born Oct. 9, 1798. Died June 16, 1873.
Feuchtwanger, Lewis, New York, N. Y. (11). Died in 1876.
Fillmore, Millard, Buffalo, N. Y. (7). Born Jan. 7, 1800. Died March 8,
Fisher, Mark, Trenton, N. J. (10). [1874.
Fitch, Alexander, Hartford, Conn. (1). Born March 25, 1799. Died Jan. 20, 1859.
Fitch, O. H., Ashtabula, Ohio (7). Died Sept. 17, 1882, in his 80th year.
Forbush, E. B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4). Born Nov. 26, 1790. Died Jan. 23, 1868.
Ford, A. C., Nashville, Tenn. (26).
Forshey, Caleb G., New Orleans, La. (21). Died in Aug., 1881.
Foster, John W., Chicago, Ill. (1). Born March 4, 1815. Died June 29, 1873.
Foucon, Felix, Madison, Wis. (18).
Fowle, Wm. B., Boston, Mass. (1). Born Oct. 17, 1795. Died Feb. 6,
Fox, Charles, Grosse Ile, Mich. (7). [1865.
Frazer, John F., Philadelphia, Pa. (1). Born July 8, 1812. Died Oct. 14,
French, J. W., West Point, N. Y. (11). [1872.

Garber, A. P., Columbia, Pa. (29). Died Aug. 26, 1881.
Gavit, John E., New York, N. Y. (1).
Gay, Martin, Boston, Mass. (1). Died Jan. 12, 1850, aged 46.
Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, W. M., Schenectady, N. Y. (10). Born in 1816. Died Jan'y 1, 1868.
Gilmor, Robert, Baltimore, Md. (1).
Glazier, W. W., Key West, Fla. (29). Died Dec. 11, 1880.
Goldmark, J., New York, N. Y. (29). Died in April, 1882.
Gould, Augustus A., Boston, Mass. (11). Born April 23, 1805. Died Sept. 15, 1866.
Gould, B. A., Boston, Mass. (2). Born June 15, 1787. Died Oct. 24, 1859.

Graham, James D., Washington, D. C. (1). Born in 1799. Died Dec. 28, 1865.
Gray, Alonzo, Brooklyn, N. Y. (13). Born in 1808. Died March 10, 1860.
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D., Boston, Mass. (1). Died Oct. 14, 1862, aged 68.
Greene, Everett W., Madison, N. J. (10). Died in 1864.
Greene, Samuel, Woonsocket, R. I. (9). Died in 1868.
Greer, James, Dayton, Ohio (20).
Griffith, Robert E., Philadelphia, Pa. (1).
Griswold, John A., Troy, N. Y. (19). Born in 1822. Died Oct. 31, 1872.
Guest, William E., Ogdensburg, N. Y. (6).

Hackley, Charles W., New York, N. Y. (4). Born March 9, 1809. Died January 10, 1861.
Hadley, George, Buffalo, N. Y. (6). Born June, 1813. Died Oct. 16, 1877.
Haldeman, S. S., Chickies, Pa. (1). Died Sept. 10, 1880, aged 68.
Hale, Enoch, Boston, Mass. (1). Born Jan. 29, 1790. Died Nov. 12, 1848.
Hance, Ebenezer, Fallsington P. O., Pa. (7). Died in 1876.
Hare, Robert, Philadelphia, Pa. (1). Born Jan. 17, 1781. Died May 15, 1858.
Harlan, Joseph G., Haverford, Pa. (8).
Harlan, Richard, Philadelphia, Pa. (1).
Harris, Thaddeus W., Cambridge, Mass. (1). Born Nov. 12, 1795. Died Jan. 16, 1856.
Harrison, A. M., Plymouth, Mass. (29).
Harrison, Jos., jr., Philadelphia, Pa. (12).
Hart, Simeon, Farmington, Conn. (1). Died April 20, 1853, aged 57.
Hartt, Charles F., Ithaca, N. Y. (18). Born in 1840. Died March 18, 1878.
Haven, Joseph, Chicago, Ill. (17). Born Jan. 4, 1816. Died May 23, 1874.
Hawes, George W., Washington, D. C. (23). Born Dec. 31, 1848. Died June 22, 1882.
Hayden, H. H., Baltimore, Md. (1).
Hayward, James, Boston, Mass. (1). Died July 27, 1866, aged 80.
Henry, Jos., Washington, D. C. (1). Born Dec. 17, 1797. Died May 13, 1878.
Hickox, S. V. R., Chicago, Ill. (17). Died in 1872.
Hilgard, Theo. C., St. Louis, Mo. (17). Born Feb. 28, 1828. Died Mch. 5, 1875.
Hincks, William, Toronto, C. W. (11).
Hitchcock, Edward, Amherst, Mass. (1). Born May 24, 1793. Died Feb. 27, 1864.
Hodgson, W. B., Savannah, Ga. (10). Born 1815.
Holbrook, John E., Charleston, S. C. (1). Born Dec. 31, 1796. Died Sept. 8, 1871.
Hopkins, Albert, Williamstown, Mass. (19). Born July 14, 1807. Died May 25, 1872.
Hopkins, James G., Ogdensburg, N. Y. (10). Died in 1860.
Hopkins, T. O., Williamsville, N. Y. (10). Died in 1866.
Hopkins, Wm., Lima, N. Y. (5.) Died in March, 1867.

Horton, C. V. R., Chaumont, N. Y. (10). Died in 1862.
Horton, William, Craigville, N. Y. (1).
Hosford, Benj. F., Haverhill, Mass. (13). Died in 1864.
Houghton, Douglas, Detroit, Mich. (1). Born Sept. 21, 1809. Died Oct. 13, 1845.
Hovey, Edmund O., Crawfordsville, Ind. (20). Born July 15, 1801. Died March 10, 1877.
Howland, Theodore, Buffalo, N. Y. (15).
Hubbert, James, Richmond, Province of Quebec (16). Died in 1868.
Hunt, Edward B., Washington, D. C. (2). Born June 15, 1822. Died Oct. 2, 1863.
Hunt, Freeman, New York, N. Y. (11). Born March 21, 1804. Died March 2, 1858.

Ives, Moses B., Providence, R. I. (9). Died in 1857.
Ives, Thomas P., Providence, R. I. (10).

Jackson, Charles T., Boston, Mass. (1). Born June 21, 1805. Died Aug. 29, 1880.
James, Thomas Potts, Cambridge, Mass. (22). Born Sept. 1, 1803. Died Feb. 22, 1882.
Johnson, Walter R., Washington, D. C. (1). Born June 21, 1794. Died April 26, 1852.
Jones, Catesby A. R., Washington, D. C. (8).
Jones, James H., Boston, Mass. (28).

Kedzie, W. K., Oberlin, Ohio (25).
Keely, George W., Waterville, Me. (1). Died in 1878.
Keep, N. C., Boston, Mass. (13). Died in 1875.
Kennicott, Robert, West Northfield, Ill. (12). Born Nov. 13, 1835. Died in 1866.
King, Mitchell, Charleston, S. C. (3). Born June 8, 1783. Died in 1862.
Klippart, John H., Columbus, Ohio (17). Died October, 1878.
Knickerbocker, Charles, Chicago, Ill. (17). Died in 1873.
Knight, J. B., Philadelphia, Pa. (21). Died March 10, 1879.

Lacklan, R., Cincinnati, Ohio (11).
Lapham, Increase A., Milwaukee, Wis. (3). Born Mch. 7, 1811. Died
LaRoche, R., Philadelphia, Pa. (12). [Sept. 14, 1875.
Lasel, Edward, Williamstown, Mass. (1). Born Jan. 21, 1809. Died in 1852.
Lawford, Frederick, Montreal, Canada (11). Died in 1866.
Lederer, Baron von, Washington, D. C. (1).
Lieber, Oscar M., Columbia, S. C. (8). Born Sept. 8, 1830. Died June
Lincklaen, Ledyard, Cazenovia, N. Y. (1). [27, 1862.
Linsley, James H., Stafford, Conn. (1). Born May 5, 1787. Died Dec. 26, 1843.
Lockwood, Moses B., Providence, R. I. (9). Died in 1872.

Logan, William E., Montreal, Canada (1). Born April 23, 1798. Died June 22, 1875.
Loosey, Charles F., New York, N. Y. (12).
Lothrop, Joshua R., Buffalo, N. Y. (15).
Lyon, Sidney S., Jeffersonville, Ind. (20). Born Aug. 4, 1808. Died June 24, 1872.

Maack, G. A., Cambridge, Mass. (18).
Mahan, Dennis H., West Point, N. Y. (9). Born April 2, 1802. Died Sept. 16, 1871.
Marsh, Dexter, Greenfield, Mass. (1).
Marsh, James E., Roxbury, Mass. (10).
Mather, William W., Columbus, Ohio (1). Born May 24, 1804. Died Feb. 27, 1859.
Maude, John B., St. Louis, Mo. (27). Died in April, 1879.
Maupin, S., Charlottesville, Va. (10).
McMahon, Mathew, Albany, N. Y. (11).
M'Conihe, Isaac, Troy, N. Y. (5).
Meade, George G., Philadelphia, Pa. (15). Born Dec. 30, 1815. Died Nov. 6, 1872.
Meek, F. B., Washington, D. C. (6). Born December 10, 1817. Died December 21, 1876.
Meigs, James Aitken, Philadelphia, Pa. (12). Born July 30, 1829. Died Nov. 9, 1879.
Minifie, William, Baltimore, Md. (12). Born in 1805. Died Oct. 24, 1880.
Mitchel, O. M., Cincinnati, Ohio (3). Born Aug. 28, 1810. Died Oct. 30, 1862.
Mitchell, William, Poughkeepsie, N. Y. (2). Died April 2, 1869, aged 76.
Mitchell, Wm. H., Florence, Ala. (17).
Morgan, Lewis H., Rochester, N. Y. (10). Born Nov. 21, 1818. Died Dec. 17, 1881.
Morris, John B., Nashville, Tenn. (26).
Morton, Samuel G., Philadelphia, Pa. (1). Born Jan. 26, 1799. Died May 15, 1851.
Mudge, Benjamin F., Manhattan, Kansas (25). Born Aug. 11, 1817. Died Nov. 21, 1879, aged 62.
Munroe, Nathan, Bradford, Mass. (6). Born May 16, 1804. Died July 8, [1866.
Munroe, William, Concord, Mass. (18). Died April 27, 1877.
Mussey, William H., Cincinnati, Ohio (30). Born Sept. 13, 1818. Died Aug. 1, 1882.

Newland, John, Saratoga Springs, N. Y. (28). Died Jan. 18, 1880.
Newton, E. H., Cambridge, N. Y. (1).
Nichols, Charles A., Providence, R. I. (17). Born Jan. 4, 1826. Died Oct. 20, 1877.
Nicholson, Thomas, New Orleans, La. (21).

Nicollett, Jean N., Washington, D. C. (1). Born July 24, 1786. Died Sept. 11, 1843.
Norton, John P., New Haven, Conn. (1). Born in July, 1822. Died Sept. 5, 1852.
Noyes, J. O., New Orleans, La. (21).
Nutt, Cyrus, Bloomington, Ind. (20). Died in 1875.

Oakes, Wm., Ipswich, Mass. (1). Born July 1, 1799. Died July 31, 1848.
Ogden, Robert W., New Orleans, La. (21). Died March 24, 1878.
Ogden, William B., High Bridge, Westchester Co., N. Y. (17). Born in 1805. Died Aug. 3, 1877.
Olmsted, Alexander F., New Haven, Conn. (4). Born Dec. 20, 1822. Died May 5, 1853.
Olmsted, Denison, New Haven, Conn. (1). Born June 18, 1791. Died May 13, 1859.
Olmsted, Denison, jr., New Haven, Conn. (1). Born Feb. 16, 1824. Died Aug. 15, 1846.
Otis, George Alexander, Washington, D. C. (10). Born Nov. 12, 1830. Died Feb. 23, 1881.

Painter, Jacob, Lima, Pa. (23). Died in 1876.
Painter, Minshall, Lima, Pa. (7).
Parker, Wilbur F., West Meriden, Conn. (23). Died in 1876.
Parkman, Samuel, Boston, Mass. (1). Died Dec. 15, 1854, aged 38.
Payn, Charles H., Saratoga Springs, N. Y. (28). Born May 16, 1814. Died Dec. 20, 1881.
Peirce, Benjamin, Cambridge, Mass. (1). Born April 4, 1809. Died Oct. 6, 1880.
Perkins, George R., Utica, N. Y. (1). Born May 3, 1812. Died Aug. 22, 1876.
Perkins, Henry C., Newburyport, Mass. (18). Born Nov. 13, 1804. Died Feb. 2, 1873.
Perry, John B., Cambridge, Mass. (16). Died Oct. 3, 1872, aged 52.
Perry, M. C., New York, N. Y. (10).
Piggot, A. Snowden, Baltimore, Md. (10).
Plumb, Ovid, Salisbury, Conn. (9).
Pope, Charles A., St. Louis, Mo. (12). Born May 15, 1818. Died July 6, 1870.
Porter, John A., New Haven, Conn. (14). Born March 15, 1822. Died Aug. 25, 1866.
Pourtalès, Louis François de, Cambridge, Mass. (1). Born in 1822. Died July 18, 1880.
Pruyn, John V. L., Albany, N. Y. (1). Born June 22, 1811. Died Nov. 21, 1877.
Pugh, Evan, Centre Co., Pa. (14).
Putnam, Mrs. F. W., Cambridge, Mass. (19). Born Dec. 29, 1838. Died March 10, 1879.
Putnam, J. Duncan, Davenport, Iowa. (27). Born Oct. 18, 1855. Died Dec. 10, 1881.

Read, Ezra, Terre Haute, Ind. (20). Died in 1877.
Redfield, William C., New York, N. Y. (1). Born March 26, 1789. Died Feb. 12, 1857.
Resor, Jacob, Cincinnati, Ohio (8). Died in 1871.
Robb, James, Fredericton, N. B. (4).
Robinson, Coleman T., Buffalo, N. Y. (15).
Rockwell, John A., Norwich, Conn. (10). Born August 27, 1803. Died February 10, 1861.
Rogers, Henry D., Glasgow, Scotland (1). Born 1809. Died May 29, 1866.
Rogers, James B., Philadelphia, Pa. (1). Born February 22, 1803. Died June 15, 1852.
Rogers, William Barton, Boston, Mass. (1). Born Dec. 7, 1804. Died May 30, 1882.
Root, Elihu, Amherst, Mass. (25). Born Sept. 14, 1845.

Sager, Abram, Ann Arbor, Mich. (6). Born December 22, 1810. Died August 6, 1877.
Schaeffer, Geo. C., Washington, D. C. (1). Died in 1873.
Schley, William, New York, N. Y. (28). Died in 1882.
Scott, Joseph, Dunham, C. E. (11). Died in 1865.
Senter, Harvey S., Aledo, Ill. (20). Died in 1875.
Seward, William H., Auburn, N. Y. (1). Born May 16, 1801. Died Oct. 10, 1872.
Sheppard, William, Drummondville, Province of Quebec, Can. (11). Born in 1783. Died in 1867.
Sherwin, Thomas, Dedham, Mass. (11). Born March 26, 1799. Died July 23, 1869.
Silliman, Benjamin, New Haven, Conn. (1). Born August 8, 1779. Died November 22, 1864.
Skinner, John B., Buffalo, N. Y. (15). Died in 1871.
Slack, J. H., Philadelphia, Pa. (12).
Smith, David P., Springfield, Mass. (29). Born Oct. 1, 1830. Died Dec.
Smith, J. V., Cincinnati, Ohio (5). [26, 1880.
Smith, James Y., Providence, R. I. (9). Born September 15, 1809. Died in 1876.
Smith, Lyndon A., Newark, N. J. (9). Born November 11, 1795. Died December 15, 1865.
Snell, Ebenezer S., Amherst, Mass. (2). Born October 7, 1801. Died in September, 1877.
Sparks, Jared, Cambridge, Mass. (2). Born May 10, 1789. Died March 14, 1866.
Spinzig, Charles, St. Louis, Mo. (27). Died Jan. 22, 1882.
Stimpson, William, Chicago, Ill. (12). Died May 26, 1872.
Stone, Samuel, Chicago, Ill. (17). Born Dec. 6, 1798. Died May 4, 1876.
St. John, Joseph S., Albany, N. Y. (28). Died Nov. 23, 1882.
Sullivant, Wm. S., Columbus, Ohio (7). Born Jan. 15, 1803. Died April 30, 1873.
Swain, James, Fort Dodge, Iowa (21). Born in 1816. Died in 1877.

Tallmadge, James, New York, N. Y. (1). Born Jan. 20, 1778. Died Oct. 3, 1853.
Taylor, Richard C., Philadelphia, Pa. (1). Born January 18, 1789. Died November 26, 1851.
Tenney, Sanborn, Williamstown, Mass. (17). Born in January, 1827. Died July 11, 1877.
Teschemacher, J. E., Boston, Mass. (1). Died Nov. 9, 1853, aged 63.
Thompson, A. Remsen, New York, N. Y. (1). Died in Oct., 1879.
Thompson, Alexander, Aurora, N. Y. (1).
Thompson, Zadock, Burlington, Vt. (1). Born May 23, 1796. Died Jan.
Thurber, Isaac, Providence, R. I. (9). [19, 1856.
Tillman, Samuel D., Jersey City, N. J. (15). Died in 1875.
Tolderoy, James B., Fredericton, N. B. (11).
Torrey, John, New York, N. Y. (1). Born August 15, 1796. Died March 10, 1873.
Torrey, Joseph, Burlington, Vt. (2). Born Feb. 2, 1797. Died Nov. 26, 1867.
Totten, Joseph G., Washington, D. C. (1). Born August 23, 1788. Died April 22, 1864.
Townsend, Howard, Albany, N. Y. (10). Died in 1867.
Townsend, John K., Philadelphia, Pa. (1).
Townsend, Robert, Albany, N. Y. (9). Died in 1866.
Troost, Gerard, Nashville, Tenn. (1). Born March 15, 1776. Died Aug. 14, 1850.
Tuomey, Michael, Tuscaloosa, Ala. (1). Born September 29, 1805. Died March 20, 1857.
Tyler, Edward R., New Haven, Conn. (1). Died Sept. 28, 1848.

Vancleve, John W., Dayton, Ohio (1).
Vanuxem, Lardner, Bristol, Pa. (1).
Vaux, William S., Philadelphia, Pa. (1). Born May 19, 1811. Died May 5, 1882.

Wadsworth, James S., Genesee, N. Y. (2). Born October 30, 1807. Died May 8, 1864.
Wagner, Tobias, Philadelphia, Pa. (9).
Walker, Joseph, Oxford, N. Y. (10).
Walker, Sears C., Washington, D. C. (1). Born March 28, 1805. Died January 30, 1853.
Walker, Timothy, Cincinnati, Ohio (4). Born Dec. 1, 1802. Died Jan. 15, 1856.
Walsh, Benjamin D., Rock Island, Ill. (17).
Wanzer, Ira, Brookfield, Conn. (18). Born April 17, 1796. Died March 5, 1879.
Warren, Gouverneur Kemble, Newport, R. I. (12). Died Aug. 8, 1882, in his 64th year.
Warren, John C., Boston, Mass. (1). Born Aug. 1, 1778. Died May 4, 1856.

Watertown, Charles, Wakefield, Eng. (1).
Watkins, Samuel, Nashville, Tenn. (26).
Watson, J. Craig, Ann Arbor, Mich. (13). Born Jan. 28, 1838. Died Nov. 23, 1880.
Webster, Horace B., Albany, N. Y. (1). Died Dec. 8, 1843.
Webster, J. W., Cambridge, Mass. (1). Died Aug. 30, 1850, aged 57.
Webster, M. H., Albany, N. Y. (1).
Weed, Monroe, Wyoming, N. Y. (6). Died in 1867.
Welch, Mrs. G. O., Lynn, Mass. (21). Died in June, 1882.
Weyman, George W., Pittsburgh, Pa. (6). Born April, 1832. Died July 16, 1864.
Wheatley, Charles M., Phœnixville, Pa. (1). Died May 6, 1882.
Wheatland, Richard H., Salem, Mass. (13). Born July 6, 1830. Died Dec. 21, 1863.
Wheeler, Arthur W., Baltimore, Md. (29). Born in March, 1859. Died Jan. 6, 1881.
White, Samuel S., Philadelphia, Pa. (23). Died Dec. 30, 1879.
Whiting, Lewis E., Saratoga Springs, N. Y. (28). Born March 7, 1815. Died Aug. 2, 1882.
Whitman, Wm. E., Philadelphia, Pa. (23). Died in 1875.
Whitney, Asa, Philadelphia, Pa. (1). Born Dec. 1, 1791. Died June 4, 1874.
Whittlesey, Charles C., St. Louis, Mo. (11). Died in 1872.
Willard, Emma, Troy, N. Y. (15). Born Feb. 23, 1787. Died April 15, 1870.
Williamson, R. S., San Francisco, Cal. (12).
Wilson, W. C., Carlisle, Pa. (12).
Winlock, Joseph, Cambridge, Mass. (5). Born Feb. 6, 1826. Died June 11, 1875.
Woodbury, Levi, Portsmouth, N. H. (1). Born Dec. 22, 1789. Died Sept. 4, 1851.
Woodman, John S., Hanover, N. H. (11). Born in 1819. Died May 15, 1871.
Wright, John, Troy, N. Y. (1).
Wyman, Jeffries, Cambridge, Mass. (1). Born Aug. 11, 1814. Died Sept. 4, 1874.

Yarnall, M., Washington, D. C. (26). Born in 1817. Died Jan. 27, 1879.
Young, Ira, Hanover, N. H. (1). Died Sept. 14, 1858, aged 57.

ADDRESS

BY

PROFESSOR GEORGE J. BRUSH,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

A SKETCH OF THE PROGRESS OF AMERICAN MINERALOGY.

MR. PRESIDENT, AND FELLOW MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

THE change in the Constitution effected at our last meeting, extending the scope of the Association and dividing it into nine sections, each with a Vice President, whose duty it is to deliver an address to the section over which he presides, has relieved the retiring President from attempting a general review of the progress of science during the past year.

I turn, therefore, to a more special subject, and invite your attention this evening to a sketch of the progress of American Mineralogy, since the commencement of this century, with particular reference to the labors of some of the early workers in the science on this continent.

During the last quarter of the eighteenth century, while great activity existed and rapid advance was made in the study of chemistry and mineralogy in Europe, almost nothing was accomplished in this new country. It is true that students in other departments of science, especially members of the medical profession, in the cities of Philadelphia, New York and Boston, attempted to arouse an interest in mineralogy, believing that the diffusion of a knowledge of this science would be of the utmost importance in the material development of the country. There were, however, no text-

books to aid the inquirer. There were no collections of minerals to stimulate the student. In the absence of these it was almost impossible that an interest in this science should be fostered, or that a spirit of investigation should be awakened.

As the first distinct beginning of the science, I may mention an association formed in 1798 in the city of New York, which assumed, as they expressed it, "the name and style of the American Mineralogical Society[1]. It announced as its object "the investigation of the Mineral and Fossil bodies which compose the Fabric of the Globe, and more especially for the Natural and Chemical History of the Minerals and Fossils of the United States." The distinguished Dr. Samuel Latham Mitchill, who seems to have been a man of universal genius, was at once its first president, its librarian, and its cabinet-keeper. The committee of the society issued a circular, in which, while expressing themselves, "desirous of obtaining and diffusing by every means in their power a correct and extensive knowledge of the mineral treasures of their country, they earnestly solicited their fellow-citizens to communicate to them on all mineralogical subjects, but especially on the following," viz.:

"1. Concerning the stones suitable to be manufactured into *gun-flints:* where are they found? and in what quantity? 2. Concerning *native brimstone or sulphur* or the waters or minerals whence it may be extracted? 3. Concerning *salt-petre:* where (if at all) found native? or the soils which produce it in the United States? 4. Concerning mines and ores of *lead:* in what places? the situation? how wide the vein? and in what kind of rock it is bedded."

This warlike demand seems to call more for the discovery of the materials for national defence than for the advancement of science, and besides being a commentary on the spirit of the times, gives a rather humorous impression of their strangely inadequate conception of the science of mineralogy, and its possible bearings on practical life. But in justice to them I should add that it is further announced that "specimens of ores, metals, coals, spars, gypsums, crystals, petrifactions, stones, earths, slates, clays, chalks, limestones, marbles, and every fossil substance that may be discovered or fall in the way of a traveller, which can throw light on the mineralogical history of America, will be exam-

[1] *The Medical Repository*, Vol. II, p. 114 (1799), edited by S. L. Mitchill and Edward Miller.

ined and analyzed without cost, sufficient pieces, with the owner's leave, being reserved for placing in the society's collection." I have quoted the circular almost verbatim to give you some idea of the genuine though crude longings for knowledge felt by our early mineralogists, and also of the generous spirit in which they worked.

A still more forcible picture of the ignorance of the time is given by the elder Professor Silliman in 1818:[2] "Notwithstanding the laudable efforts of a few gentlemen," he says, "to excite some taste for mineralogy, so little had been effected in forming collections, in kindling curiosity and diffusing information, that only fifteen years since (1803), it was a matter of extreme difficulty to obtain *among ourselves* even the *names* of the most common stones and minerals; and one might inquire earnestly and long before he could find any one to identify even *quartz*, *feldspar* or *hornblende* among the simple minerals, or *granite*, *porphyry* or *trap* among the rocks. We speak from experience, and well remember with what impatient, but almost despairing curiosity we eyed the bleak, naked ridges which impended over the valleys and plains that were the scenes of our youthful excursions. In vain did we doubt that the glittering spangles of mica, and the still more alluring brilliancy of pyrites, gave assurance of the existence of the precious metals in those substances, or that the cutting of glass by the garnet, and by quartz, proved that these minerals were the diamond; but if they were not precious metals, and if they were not diamonds, we in vain inquired of our companions, and even of our teachers, what they were."

Such, then, was the state of knowledge in mineralogy here at the commencement of the century. A few American minerals, collected by travellers from time to time, had before this been taken to Europe for identification, but among these were discovered only two minerals new to science. The Moravian missionaries found at St. Paul in Labrador, the beautiful species of feldspar called by Werner *labrador-stein*, which in more modern times we know under the name of *labradorite*. Klaproth, the most eminent analytical chemist of his time, discovered that the so-called fibrous barytes from Pennsylvania was the sulphate of the then newly discovered earth strontia.[3] He thus, for the first time, identified

[2] Review of Cleaveland's Mineralogy, *American Journal of Science* (1), I, p. 36.
[3] *Beiträge zur chemischen Kentniss der Mineralkörper*, II, 92.

the mineral species *celestite* which was subsequently found in various localities in Europe.

Although little had been accomplished in America previous to 1800, the first quarter of the new century was destined to show great development here in the study of mineralogy. During the early years of this quarter several collections of European minerals were brought to this country by American gentlemen who had availed themselves during a residence in Europe of the best opportunities for acquiring a knowledge of the science from the great masters of the subject in Germany and France. About this time also several colleges in the country had instituted chairs of chemistry and mineralogy, and a commencement was thus made in teaching these sciences in the higher schools. As the result of these influences the number of persons interested in mineralogy was largely increased, and an active search for minerals was initiated throughout all of the older United States and to a considerable extent also in Canada.

So energetically were these explorations followed up that in 1825 a Catalogue[4] of American minerals was published by Dr. Samuel Robinson, with their localities arranged geographically, and giving only such as were known to exist in the United States and the British Provinces. It formed an octavo volume of over three hundred pages.

That much credit was due to *many* workers during this period, both in the field and in the laboratory, there can be no question, but among them all I find four men standing forth so prominently as leaders, that I have thought it would be well for us to recall briefly something of the character of these men and their labors for the advancement of mineralogy in this country.

First among these I will mention Dr. ARCHIBALD BRUCE.[5] He was the son of Dr. William Bruce, a surgeon in the British army, and was born in New York in 1777. He was graduated at Columbia College; subsequently studied medicine, and in 1798 went to Edinburgh where, in 1800, he obtained his doctor's degree from that university. He was early interested in natural science and while still in college found, his biographer says, "the collection and examination of minerals — a pursuit not then at all attended

[4] *Catalogue of American Mineral Localities*, by Dr. Samuel Robinson. 8vo, pp. 316. Boston, 1825.

[5] For a biographical notice of Dr. Bruce see *American Journal of Science*, Vol. I, p. 299, 1818.

to in this country — was his particular relief from other studies; for even during his recreation he was ever on the lookout for something new or instructing in mineralogy."

When he went to Europe he took with him a large number of American minerals, and through exchanges with institutions and prominent mineralogists abroad, he established friendly relations with those most interested in his favorite science. After the completion of his medical studies, he travelled for two years on the continent of Europe, making the acquaintance of the Abbé Haüy, and other eminent mineralogists, and collecting an extensive cabinet of valuable minerals, which on his return to this country in 1803, he brought with him to New York. This collection, with another brought to New York about the same time by Mr. B. D. Perkins — both being made fully accessible to all interested in seeing them — contributed, it was said, more than any agencies had ever done before, to excite in the public an active interest in the science of mineralogy. Besides this Dr. Bruce entered into extensive correspondence with others interested in the subject, was active in visiting and discovering new mineral localities, and in advising, encouraging and inspiring young mineralogists. Finally, after well considering the matter, he established the first purely scientific periodical ever published in America. This was called the *American Mineralogical Journal*,[6] and the first number of it was published in 1810. It contained original contributions, chiefly on mineralogy, from a number of investigators. "It was received," says the elder Silliman, "in this country and in Europe in a flattering manner; it excited at *home* great zeal and effort in support of the sciences which it fostered, and *abroad* it was hailed as the harbinger of our future exertions." But, alas! it was in advance of the age, and, after struggling for several years, was given up on the publication of the fourth number. Possibly it would have continued longer had it not been for the failing health of its founder.

This Journal contained several important papers by Dr. Bruce: among them, the investigation and description of two new mineral

[6] The *American Mineralogical Journal*, being a collection of facts and observations tending to elucidate the mineralogy and geology of the United States of America; together with other information relating to mineralogy, geology and chemistry derived from scientific sources. Conducted by ARCHIBALD BRUCE, M.D., Professor of Materia Medica in the Medical Institution of New York, and Queen's College (N. J.). Vol. I, 270 pp. New York. 1814.

species, the *native magnesia* of Hoboken and the *red zinc oxide* of Sussex Co., New Jersey. These are the first American species described by an American mineralogist. So thoroughly was the work done by Bruce, that these species remain to-day essentially as he described them, and his papers may well be studied by mineralogists now as models of accuracy and clearness of statement.

Dr. Bruce did much also for the elevation of the medical profession, was one of the founders of the New York Medical Society, and was largely influential in obtaining the charter of the College of Physicians and Surgeons, in which he was subsequently the Professor of Materia Medica and Mineralogy.

He is described as a successful teacher, a man of wide acquirements and of great integrity, which qualities, with his abounding generosity and hospitality, commanded the respect and regard of all who knew him. It was a great loss to science and to his country that so able an investigator should have been cut off at the early age of 42. He died in New York, Feb. 24, 1818.

I have mentioned that the importation and exhibition of collections of minerals from Europe had contributed much to excite an interest in the study of mineralogy. It was necessary to have known minerals for study and comparison in order properly to determine those obtained by exploration here. In 1805, Colonel GEORGE GIBBS of Rhode Island, for many years a resident in Europe, returned from his travels with a collection of minerals the most extensive and valuable ever brought to America. Colonel Gibbs was a zealous cultivator of mineralogy and, fortunately for science, a young man of wealth. He used his money freely for the purchase of whole cabinets, as well as in personal explorations in search for minerals.

The larger part of his collection was made by the purchase of two famous European cabinets, one from the heirs of Gigot d'Orcy, a noted French collector, and said to have been the result of forty years' labor, the other from Count Gregoire de Razamowsky, a Russian nobleman, long resident in Switzerland. D'Orcy's cabinet numbered over 4,000 specimens, chiefly from France, Germany, Italy and Great Britain; Razamowsky's contained about 6,000 specimens from the Russian Empire and the remainder principally from Germany and Switzerland; in all, with the other col-

lections made by Colonel Gibbs, it is said that more than 20,000 specimens were brought by him to this country.[7]

In 1807, a portion of this collection was opened in Newport, and many interested in mineralogy made pilgrimages there, to view the treasures it contained. Among others was Professor Silliman, who states in his diary, that he spent many weeks in studying the minerals with Colonel Gibbs, finding in the latter "a scientific friend and a professional instructor and guide." That Colonel Gibbs reciprocated Professor Silliman's feelings of friendship there can be no doubt; for, after various offers to deposit his collection for exhibition in Boston, New York and elsewhere, to the great surprise of Professor Silliman, he proposed to open the cabinet at Yale College, provided rooms should be fitted up for its reception.

The proposition was promptly responded to by the authorities of the college, and in 1810, 1811 and 1812, under the personal supervision of Colonel Gibbs, it was opened and arranged, and generously placed at the disposition of the institution and the public. The opening of this collection in New Haven formed an important epoch in the history of the college, and gave a powerful impetus to science throughout the country. It was not only studied by the pupils of the college, but it was visited by travellers from all parts of the United States.

In 1825, the collection had for fifteen years been exhibited without any advantage to the owner, other than the satisfaction of observing the great amount of good which was effected by the knowledge it disseminated, and the enthusiasm with which it inspired students. Colonel Gibbs then offered the whole for sale, giving the college the preference as purchaser. Fortunately and mainly through the influence of Professor Silliman, the institution succeeded in raising the funds ($20,000) necessary for its purchase, and the ownership of this collection has exercised a most important influence in the development of natural science at New Haven.

Colonel Gibbs, however, did not confine himself to the collection of minerals in Europe. On his return to this country he made extensive journeys and opened up new mineral localities, giving his time and specimens freely to aid others who were interested in this special study. At Yale, as an incentive to stu-

[7] *Medical Repository* (2), V, 213 (1808).

dents, he for many years offered prizes for superiority of attainments in mineralogical knowledge and for services rendered to the science by useful discoveries and observations.[8]

He published valuable papers both in the *American Mineralogical Journal* and the *American Journal of Science*, and did much by his counsel and coöperation to support these publications. Indeed, it was from Colonel Gibbs that Professor Silliman first received the suggestion that he should institute a new Journal of Science, in order that the advantages already gained by the short-lived Mineralogical Journal might be secured and further progress for science might be made.[9]

In every way Colonel Gibbs proved himself a liberal patron of science, and it was most fortunate for the promotion of mineralogy in this country that he should so unselfishly have devoted his wealth and his personal influence to its advancement. He died August 5, 1833, aged 57.

Much as had been accomplished by the free exhibition of cabinets and the explorations and investigations of enthusiastic workers in mineralogy during the years from 1805 to 1815, a great drawback was now felt to the progress of the science from the want of text-books. Most of the literature of the subject was in German and French, but the works of the French and German authors had not then been translated and consequently were accessible only to the few who were acquainted with these languages.

In English there were not many treatises on the subject. That by Richard Kirwan, the eminent Irish mineralogist of the last century, was a renowned work in its day, but, as the last edition of it had been published in 1794, it was already too old to be of much service to the student. Jameson's treatise was somewhat more recent (1804),[10] but its great fulness and exclusive devotion to the Wernerian system made it an undesirable book for beginners, aside from the fact that its price was such that few students in those days could afford to buy it. So much progress had been made at home and abroad, that a work was needed here which should include the modern discoveries, and one also which should

[8] *American Mineralogical Journal*, p. 269.

[9] Obituary notice of Colonel Gibbs, by Professor Silliman, *American Journal of Science* (1), XXV, 214.

[10] Of Jameson's *Mineralogy*, 1st edition, the 1st volume was published in 1804, 2nd volume in 1805, 3d volume in 1808.

gather up the scattered facts already published in regard to American minerals.[11]

Fortunately for the further progress of science in this country this was done by Professor PARKER CLEAVELAND. His work was published in 1816, and was entitled "An Elementary Treatise on Mineralogy and Geology."[12]

Professor Cleaveland was professor of mathematics and natural philosophy in Bowdoin College, and, like many other professors of science in the early history of American colleges, was charged by the trustees to lecture also on mineralogy and chemistry. He was an enthusiastic student of mineralogy, was well acquainted with the literature of the science in various languages, had been a successful teacher of the subject for many years, and withal was both an explorer and investigator, and held intimate relations with the leading mineralogists of the day. The work was modelled on the general plan of Brongniart, combining the excellences of both the French and German schools, and gave in detail almost everything then known in regard to American minerals. It supplied the pressing need for a thorough, systematic and *American* treatise on mineralogy, well suited to all classes of students, and it was written in such a masterly style that it won for its author the highest praise from the leading mineralogists of the world. "It brought," says Professor Silliman, "within the reach of the American student the excellences of Kirwan, Jameson, Haüy, Brochant, Brongniart and Werner, and we are not ashamed," he says, "to have this work compared with those of these celebrated authors."[13] His biographer[14] states that "he received letters of respect and congratulation from Sir David Brewster, Sir Humphrey Davy and Dr. Macculloch, in England, from Berzelius, in Stockholm, Germar of Halle, from Brongniart, Baron Cuvier and the Abbé Haüy, in Paris."

[11] The *Manual of Mineralogy*, by A. AIKIN, was reprinted in Philadelphia in 1815, but it contained nothing in regard to American minerals.

[12] An elementary treatise on Mineralogy and Geology, being an introduction to the study of these sciences, and designed for the use of pupils,—for persons attending lectures on the subjects,—and as a companion for travellers in the United States of America. By PARKER CLEAVELAND, Professor of Mathematics and Natural Philosophy, and Lecturer on Chemistry and Mineralogy in Bowdoin College. One volume, 668 pp. 8vo, with six plates. Boston, 1816.

[13] Review of Cleaveland's Mineralogy, loc. cit.

[14] The main facts as stated in regard to Professor Cleaveland are from an address on his life and character by Leonard Woods, D. D., President of Bowdoin College. Brunswick, 1860.

The work at once took rank as one of the leading authorities on the science and was introduced as a class-book in the principal schools and colleges in America. The first edition was soon exhausted and a new and revised edition with more than a hundred pages of new matter was published in 1822. The demand was so great that this likewise was soon out of print and a third edition was called for by the public, but Professor Cleaveland had about this time become so engrossed in the administration of the affairs of the new Medical School at Brunswick that he was unable to respond to the call, having turned his thoughts and efforts in new directions.

Unfortunately for the science of mineralogy, in which he had obtained such eminence as an author and teacher, he no longer contributed actively to its progress, although he continued his work as lecturer on the science so long as he lived. He died Oct. 16, 1859, in the seventy-ninth year of his age.

The last to be mentioned of these early leaders is Professor BENJAMIN SILLIMAN. His name is so intimately associated with the progress of science on this continent during the first half of the present century, that his life work is more or less familiar to all[15]. But the important service he rendered in the early history of mineralogy deserves especial recognition here, not only for the work he himself did in the laboratory and the field, but because his enthusiasm and zeal were a constant inspiration to others.

Commencing with the historic "candle-box" of unlabelled stones which he took to Dr. Adam Seybert, of Philadelphia, to be named, he began with enthusiasm the acquisition of knowledge and the gathering of material to illustrate the mineral kingdom. During a residence in England and Scotland in 1805–6 he had opportunities to add to his information, and collect many specimens, chiefly from the mines of Derbyshire and Cornwall. On his return to America he at once applied the knowledge he had acquired in making an exploration of the mineral structure of the environs of New Haven, and read a paper on this subject to the Connecticut Academy of Arts and Sciences in September, 1806.

In the following year he induced the corporation of the college

[15] See Life of BENJAMIN SILLIMAN, M. D., LL. D., Late Professor of Chemistry, Mineralogy and Geology in Yale College. Chiefly from his manuscript reminiscences, diaries, and correspondence, by GEO. P. FISHER, Professor in Yale College. Two volumes, 12 mo. New York, 1866.

to purchase the mineral collection of Mr. B. D. Perkins of New York (already referred to), for one thousand dollars, thus placing the institution in possession of means for illustrating the science of mineralogy far in advance of anything it had before enjoyed.

The occurrence of the fall of the Weston Meteorite in December, 1807, offered an opportunity for Professor Silliman to undertake, in connection with his colleague, Professor Kingsley, an investigation into the circumstances of the phenomena, and the character of the stones which fell at that time. The results of this investigation were presented to the American Philosophical Society and published in the *American Philosophical Transactions* in 1809. The diligence employed in obtaining all the facts possible from eye-witnesses of the occurrence, and the care and skill shown in the chemical and mineralogical examination of the meteorite, made this paper one of the most remarkable memoirs of the time, and attracted the attention of philosophers throughout the world.

As already stated, it was the personal enthusiasm and magnetic influence of Professor Silliman which led Colonel Gibbs to deposit his great cabinet of minerals in New Haven, under the care of his friend. It was due to the same qualities in Professor Silliman that the college secured the permanent possession of this invaluable collection, which probably has done more than any other single agency to create an interest in and disseminate a knowledge of mineralogy in this country.

The establishment of the *American Journal of Science* in 1818, now everywhere recognized as of inestimable value to all departments of science, was peculiarly helpful to mineralogy, and the early volumes are rich in articles on this subject. Professor Silliman's original contributions to science were more in chemistry and geology, but he also is the author of several important papers on mineralogy, and was the discoverer of the occurrence of native tungstic acid as a mineral species. For more than fifty years he continued as a teacher in Yale College, and when he resigned his professorship in 1853, he had the satisfaction to have as his successor in the department of mineralogy and geology Professor James D. Dana, who was already among the foremost mineralogists of the day, and whose published works, before and since his accession to this professorship, have done so much for the advancement of mineralogy.

Professor Silliman retired from his active labors in his seventy-fourth year, still in full possession of his remarkable physical and mental powers, and lived honored and revered until Nov. 24, 1864, when he passed to his rest.

It will be inferred from what has been said of these pioneers that the developments and discoveries of minerals, during the first twenty-five years of the century, were due entirely to individual enthusiasm and private enterprise. Up to this time no aid had been received from either state or national governments, and in looking over the work accomplished during this period we are filled with wonder and admiration at the energy and rare devotion to science exhibited. The larger portion of the continent was an unbroken wilderness, and the facilities of communication even in the settled parts of the country were of the most primitive character. Yet at the present day, with our means of rapid transportation, many naturalists would hesitate to undertake the long journeys then made for purely scientific purposes.

Geologists as well as mineralogists will recall how much science is indebted to such men as William Maclure, James Pierce, Thomas Nuttall (the botanist), and others who made extensive trips through the whole territory east and in some instances to the west of the Mississippi River. Maclure not only devoted his time and money to making and publishing a geological survey of the United States and Canada, the first report of which was made in 1809,[16] but to him the Academy of Natural Sciences, in Philadelphia, owes its first endowment.[17]

I shall be pardoned, I trust, if I mention still another signal instance of private liberality in this connection. Gen. Stephen Van Rensselær, of New York, a generous patron of science, defrayed all the expenses of a geological survey of the country adjacent to the Erie Canal, including the making of a geological section from Lake Erie to the eastern coast of Massachusetts.[18] This survey

[16] Published later with additions, in a volume of 127 pp., 8vo, with two plates. Philadelphia, 1817.

[17] Memoir of William Maclure, Esq., late President of the Academy of Natural Sciences of Philadelphia, by S. G. Morton, M. D. *Am. J. Sci.*, (1) XLVII, 1 (1844.)

[18] A Geological and Agricultural Survey of the district adjoining the Erie Canal, in the State of New York, taken under the direction of Hon. Stephen Van Rensselær. Part I, containing a description of the rock formations, together with a profile extending from the Atlantic to Lake Erie. 163 pp. 8vo. Albany, 1824.

was under the charge of Professor Amos Eaton, with a competent corps of assistants, and was continued for four years, from 1820 to 1824, at a cost of many thousands of dollars. General Van Rensselær was also the founder of the first school of technical science in this country—the Rensselær Polytechnic Institute, at Troy, which was placed under the charge of Professor Eaton. It may be interesting here, in these days of summer schools, to recall, although parenthetically, that what was probably the first summer school of science in the United States was established more than fifty years ago in connection with this institution. The school consisted of a flotilla of towed canal boats, and the route was from Troy to Lake Erie. It took two months for the trip and all important points on the way were visited. Instruction by lectures and examinations was given in mineralogy, geology, botany, zoology, chemistry, experimental philosophy and practical mathematics, particularly land surveying, harbor surveying and engineering. One of the largest boats in the flotilla was fitted up as a laboratory, with cabinets in mineralogy and geology and also scientific books for reference. Students were taught the method of procuring specimens and were required to make collections of whatever was interesting on the route.[19]

The public mind was finally awakened to the importance of the work which these explorers and investigators had carried on single handed. Government now came to the aid of Science. In 1824, one State legislature, that of North Carolina, authorized a geological survey to be made. This example was followed in 1830 by Massachusetts and soon after by New York, Pennsylvania, Virginia and other states, and also by the national government, until as is now well known, the whole territory of the United States and Canada, either has been or is in the process of being surveyed. Several of the state surveys published independent volumes on the mineralogy of their respective states, and these surveys have been a powerful auxiliary in extending our knowledge of the occurrence of minerals on this continent. The opening of mines and quarries throughout the country has also furnished abundant material for study. The large number of original contributions which have been published in the volumes of State Surveys, the treatises by American authors, and the still larger number of memoirs and papers com-

[19] *American Journal of Science* (1), XVIII, 200. (1830.)

municated to our academies of science and scientific journals cannot be even enumerated in this place, neither is it my purpose to attempt to give here a list of the names of those who have been actively engaged in making researches on American minerals.[20] Still less can I attempt to give an account of the work that has been and is being done by living mineralogists. The sketch which I have presented of the four typical workers has in a measure shown the character of our early mineralogists, the earnest spirit in which they labored, and what they accomplished in the first quarter of the century. The point to which the science has reached in the last quarter of the century cannot be unfamiliar to you all.

In the time that remains I desire to call your attention to some of the developments made in the field in which our mineralogists have worked. It was thought by many scientists in the first half of this century that our rocks seemed likely to afford less variety of mineral contents than the rocks of Europe. Further study, however, and more careful and extended observations encourage us to believe that our mineral riches, even in variety of species, will compare favorably with those of other continents. Already fully one-half of the known mineral species have been found here. The present number of known minerals is variously estimated to be from seven hundred to one thousand. There have been described, as occurring here, nearly three hundred supposed new American minerals. Of these, perhaps one-quarter are new to science and the remainder have either been proved to be identical with species already described, or their characters are so imperfectly given that further investigation is needed to ascertain what they are.

Among these new minerals are some of great interest to science. Time, however, will not allow, even if your patience would permit me, to give the facts in detail; but in justice to the describers of those announced to be new, I will print, as an appendix to this address, as complete a list as I have been able to make of the names of the proposed new American mineral species, with the names of their sponsors. The list will, I trust, be instructive both as a warn-

[20] For a list of papers by American chemists, including a large proportion of papers published in mineralogy, see *American Contributions to Chemistry*, an address delivered on the occasion of the celebration of the Centennial of Chemistry at Northumberland, Pa., August 1, 1874, by BENJAMIN SILLIMAN. 175 pp. 8vo. Reprinted from the *American Chemist* for August, September and December, 1874.

ing and an encouragement to investigators. The ambition to make new species is recognized as a drawback in every department of science, and mineralogy has probably not suffered in this respect more than other sciences. Nor do I believe that American mineralogists have, as a class, been less careful in describing new species than their European confrères. There have been flagrant examples of carelessness in both hemispheres, and the growing tendency during the last ten or fifteen years to call things new which have been only imperfectly investigated cannot be too strongly censured. "If two very simple rules," says a recent writer,[21] "could be conscientiously followed by those investigating supposed new mineral species, the science of mineralogy would be vastly benefited. These are: first, that the material analyzed should in every case be proved by a careful microscopical and chemical examination to be *homogeneous;* and second, that the thorough investigation which is to establish the position of a "new species" should precede, not follow, the giving of a new name. A mineral which can only be partially described does not deserve a name.

In comparing the minerals found in America with those of Europe, although interesting minor variations are observed, it can hardly be expected that very marked differences should exist. This is, of course, due to the fact, that in the inorganic kingdom, nature has everywhere to do with the same elements, under essentially like conditions. A large number of remarkable analogies between the minerals of the two continents will occur to any one familiar with the subject, as, for example, the character of the occurrence of individual minerals in the rocks of the northeastern United States and Canada as compared with those of Norway and Sweden, and numberless instances of like association of minerals in various parts of Europe find their counterparts here.

A marked feature of American minerals is the grand scale upon which crystallization has taken place, individual crystals of large size being very common. The granite veins of New England afford striking examples of this kind. We have common mica, in sheets a yard across; feldspar has been observed where a single cleavage plane measured ten feet; gigantic hexagonal prisms of beryl, four feet long and more than two feet in diameter, and

[21] Preface to the Third Appendix to the fifth edition of Dana's *Mineralogy*. New York. 1882.

weighing over two tons, have been described; spodumene crystals six to seven feet in length and a foot or more across, and masses of rock crystal of immense size, have been found. Canada and New York have given crystals of apatite, phlogopite and spinel which for these species are of marvellous grandeur in dimensions. Many other American localities might be mentioned where large crystals occur. While it is true that these are extraordinary instances, it is also true as a general fact, common to a very large proportion of the minerals found in this country, that the species occur in much larger crystals than those obtained from European localities.

Another point worthy of note is the occurrence in comparatively large quantities, and over wide areas, of some of the rarer elements as constituents of the minerals found. In illustration of this we have, among the rare earths, *glucina* combined with silica and alumina in the mineral *beryl*, occurring in large quantity and perhaps in a hundred or more places; *zirconia*, in the mineral *zircon*, is also very widespread in its range of occurrence as an original constituent of the older rocks, as well as a vein mineral; localities are known which have furnished this rare species by the hundred weight. The *cerium* earths are found largely in the mineral *allanite*, which occurs in so many places that it may be said to be a common mineral in the United States. These earths are also found in the rare phosphate *monazite*, a mineral that in America has a wide range of localities, and recently this species has been found in crystals of two, three, and, in one instance, of eight pounds in weight. Again, three new earth metals, *mosandrum*, *phillipium* and *decipium* have been described as occurring with the cerium earths and yttria in the North Carolina *samarskite*. The rare alkali metal *lithium*, sometimes associated with the still rarer metals *rubidium* and *cæsium*, is found not only of widespread occurrence in our lithia micas, but the mineral *spodumene*, containing from five to eight per cent of lithia, occurs by the ton in at least one locality and must be looked upon as one of the common American minerals, being found in the granite veins in Maine, New Hampshire, Massachusetts and Connecticut, and as far south as North Carolina and Georgia. Lithia also is one of the constituents of the phosphate *triphilite*, and there are several localities known where this mineral occurs abundantly. Again, we

have the frequent occurrence of some of the rare metals which form metallic acids: *Columbium*, the first metal, new to science, discovered in America, associated with its twin metal *tantalum*, is found in columbite in our granite veins from Maine to Georgia, a range of more than a thousand miles, in a score or more of localities, and sometimes is obtained by the hundred weight at a single locality. The American variety of *samarskite*, another rare columbate, has also been found in masses of fifty pounds or more in weight, and these acids occur in still other American species. *Molybdenum*, both as sulphide and in the oxidized form as native molybdic acid and molybdate of lead, is found in many localities, and occasionally in large quantity. Quite recently *vanadium* compounds have been discovered in several places, and *tungstates* have also been observed over a wide range of country. *Titanium* has been found in enormous quantities in extensive deposits of titanic iron as well as in the form of rutile and in sphene. The rare metal *tellurium* occurs native in Colorado in one locality where single masses of twenty-five pounds in weight have been taken out, and several new tellurium compounds have been found in our western mines.

It is, perhaps, unnecessary to enumerate more fully the many occurrences of other rare elements in American minerals. Enough has already been said to show that important developments have been made in the discovery and investigation of the minerals found in our American rocks during the past eighty years. Nevertheless it is but a commencement in the work. Only a very small portion of our territory has been explored with any thoroughness, and none of it exhaustively. The enormous production of the precious metals, and the extensive deposits of ores of the more common metals which have been opened up during the past twenty or thirty years, have placed us in the front rank as metal producers, but we are still far behind Europe in the variety of minerals obtained from our mines. This may be due, in some instances, to the character of the veins or ore deposits, there being, as in many of our gold and silver mines, remarkably few associated minerals. In other cases, however, it is doubtless due to the fact that very few persons connected with our mines have even an elementary knowledge of the rudiments of mineralogy, while in continental Europe almost every mining officer is familiar with all

the ordinary minerals. Thanks to the training of our schools of science, an improvement in this respect is already noticeable, as is shown in the discoveries made in the mines of our Western States and Territories during the past few years.

While the service done for mineralogy by our geological surveys is gratefully acknowledged, we feel we have a right to demand much more from them in the future. Mineralogy has been too largely looked upon as a guide to the discovery of useful ores and minerals and not as a matter for scientific study; fortunately during the past decade the discoveries in optical mineralogy, and their importance in the determination of the constituent minerals of the crystalline rocks, have led many geologists to again recognize the desirability of a knowledge of our science. Much will be accomplished if those in charge of geological surveys will direct competent persons to make observations, not only on the main mineral constituents of rocks but also on the manner of occurrence of individual minerals. The careful inspection of quarries and mines is greatly to be desired. These are rich sources for minerals; but unless constant watchfulness is exercised, valuable material for science is in danger of being buried out of sight.

It is too true that many of the most interesting discoveries already recorded seem to have been due more to the result of fortunate accident than of systematic and intelligent exploration. If our trained mineralogists, instead of devoting most of their attention to the examination of specimens in cabinets collected by others, would give more time to personal observation in the field in the study of the order and manner of occurrence of mineral species in place, our knowledge would doubtless be greatly promoted. Again, if our wealthy amateurs could be induced to spend their money as freely in the exploration of promising American localities as in the importation of costly European specimens, we might hope for many important discoveries, and they could have the satisfaction not only of gaining novelties for their collections, but incidentally they would do much to foster science.

In order to keep pace with the progress of the science, we need many more workers who will devote themselves especially to mineralogical research, and we need more of the spirit of the early

workers. It is my belief that the number of persons at present interested in the study here, either as amateurs or investigators, is relatively less than in 1825. The mineralogy of to-day is a very different subject from the mineralogy of the commencement of the period over which we have so hastily glanced. Then the study of minerals was confined almost exclusively to their external characters. Led by Werner[22] and reinforced by his most gifted pupil Mohs, the majority of mineralogists claimed mineralogy to be a purely natural history science. They gave their attention, as has been well said, entirely to "*how the mineral looked*," and not at all to "*what it was*." On the other hand the development of analytical chemistry by the labors of Klaproth and Berzelius led many to take up mineralogy from a purely chemical standpoint. These two schools working independently brought great confusion into the science. The discoveries of Haüy in crystallography, and especially his labors in establishing a mathematical foundation for the geometrical form of crystals, and the recognition that the constancy of form depended on the constancy of the "integrant molecule," were steps which paved the way for modern mineralogy. In this a union of all the physical, geometrical and chemical properties is required in order to determine the true character of a mineral.

Further, we are called upon to investigate the history of its origin, its relation to associated species, the changes which it undergoes, and the causes and results of those changes. Here we have to do largely with both geology and chemistry. From this it becomes evident that a much broader foundation is now required for the mineralogist than in the early days of the century. The bearing of physics, geology and chemistry in the study of the mineral kingdom must be thoroughly recognized and appreciated by every investigator who desires to contribute to further progress. No mineralogist can expect to have a profound knowledge in all these directions, but he must be at least capable of intelligently applying to his subject the results obtained by experts in these sciences. Mineralogy is deeply indebted to

[22] Werner, although using physical characters in the main, and entirely in the determination of minerals, recognized chemistry in arranging minerals in families or groups, but Mohs, Jameson and others founded their systems "totally independent of any aid from chemistry." (Preface, Jameson's *Mineralogy*, 3d ed., Vol. I, p. iii.)

special investigators in all these departments. Without their co-operation it would have been impossible to discover the relations of form and other physical characters with that fundamental arrangement of molecules whose nature it is now admitted controls all the properties of a substance.

The study of natural crystals has yielded rich material for the physicist. In the department of optics it has given results from which many fundamental laws have been deduced; and natural crystals, too, have furnished, in many cases, the very apparatus which made investigations possible. Some chemists claim that mineralogy is not at all a science by itself, and constitutes only a small part of inorganic chemistry. It can be unquestionably conceded that a knowledge of chemistry is fundamental, and in consequence this claim has a certain plausibility. On the other hand, we contend that it was largely the labors of the mineralogists on the physical characters of minerals, and especially their demonstration of the relation of form to chemical composition which finally awakened chemists to a more profound study of their own subject. The law of isomorphism was discovered by a chemist, whose training as an expert crystallographer in the examination of natural crystals made it possible for him to recognize the wonderful relation of form to composition. Dimorphism was first established from observations made on minerals, and it is in the study of the mineral kingdom that the laws of isomorphism and dimorphism find abundant demonstration. From the further investigation of the chemical nature of minerals we may hope for new light on the molecular constitution of substances which as yet the chemist has been unable to reproduce. We have already indicated the interdependence of geology and mineralogy. May we not claim the same interdependence of mineralogy, physics and chemistry, letting each go on in its own sphere, contributing to the general progress, sure that every new fact observed and every new law discovered will be for the common advancement of all?

APPENDIX.

A List[23] *of Names given to Minerals from the United States and Canada, for the most part described as New Species.*

Acadialite (= Acadiolite, *T. Thomson*)	*F. Alger* and *C. T. Jackson.*
Adamsite	*C. U. Shepard.*
Aglaite	*A. A. Julien.*
Alaskaite	*G. A. König.*
Albertite	*C. Robb.*
Algerite	*T. S. Hunt.*
Ambrosine	*C. U. Shepard.*
Amesite	*C. U. Shepard.*
Animikite	*H. Wurtz.*
Annite	*J. D. Dana.*
Anthophyllite, hydrous	*T. Thomson.*
Apatoid (*m*)[24]	*C. U. Shepard.*
Aquacreptite	*C. U. Shepard.*
Aragotite	*F. E. Durand.*
Arkansite	*C. U. Shepard.*
Baltimorite	*T. Thomson.*
Barnhardite	*F. A. Genth.*
Barytocelestite (= baryto sulphate of strontia	*T. Thomson.*
Beaumontite	*A. Lévy.*
Begeerite	*G. A. König.*
Bernhardinite	*J. M. Stillman.*
Bismuthaurite	*C. U. Shepard.*
Blackmorite	*A. C. Peale.*
Boltonite	*C. U. Shepard.*
Bowenite	*J. D. Dana.*
Brucite (= Native Magnesia, *Bruce*)	*F. S. Beudant.*

[23] This list is compiled almost entirely from the *Fifth edition of Dana's Mineralogy, and Appendices I–III to the same work*, in which will be found references as to where these minerals were originally described, as also any further facts published in regard to them, and their present position in the science.

[24] (*m*) signifies that the substance was found in a meteorite.

Brucite (Chondrodite) . . . . . *G. Gibbs.*
Byerite . . . . . *J. W. Mallet.*
Bytownite . . . . . *T. Thomson.*

Calaverite . . . . . *F. A. Genth.*
Calcimangite . . . . . *C. U. Shepard.*
Calcozincite . . . . . *C. U. Shepard.*
Calstronbarite . . . . . *C. U. Shepard.*
Calyptolite . . . . . *C. U. Shepard.*
Campbellite (*m*) . . . . . *St. Meunier.*
Canaanite . . . . . *S. L. Dana.*
Cantonite . . . . . *N. A. Pratt.*
Carrollite . . . . . *W. L. Faber.*
Cassinite . . . . . *I. Lea.*
Catlinite . . . . . *C. T. Jackson.*
Celestialite (*m*) . . . . . *J. L. Smith.*
Celestite (= schwefelsaurer Strontianit aus Pennsylvanien, *Klaproth*, 1797) . *A. G. Werner.*
Centrallasite . . . . . *H. How.*
Cerinite . . . . . *H. How.*
Chalcodite . . . . . *C. U. Shepard.*
Chalcophanite . . . . . *G. E. Moore.*
Chathamite . . . . . *C. U. Shepard.*
Chelmsfordite . . . . . *J. F.* and *S. L. Dana.*
Cherokine . . . . . *C. U. Shepard.*
Chesterlite . . . . . *T. F. Seal.*
Chiltonite . . . . . *E. Emmons.*
Chladnite (*m*) . . . . . *C. U. Shepard.*
Chlorastrolite . . . . . *C. T. Jackson* and *J. D. Whitney.*
Chlorophyllite . . . . . *C. T. Jackson.*
Chromchlorite . . . . . *R. Hermann.*
Chrysophane (= Seybertite) . . . . *A. Breithaupt.*
Cleavelandite . . . . . *H. J. Brooke.*
Cleiophane . . . . . *T. Nuttall.*
Clingmanite . . . . . *B. Silliman, Jr.*
Clinochlore . . . . . *W. P. Blake.*
Clintonite (= Seybertite) . . . . . *W. W. Mather.*
Coloradoite . . . . . *F. A. Genth.*
Columbite (= ore of Columbian, *C. Hatchett*, 1802) . . . . . *R. Jameson.*
Cookeite . . . . . *G. J. Brush.*
Copperasine . . . . . *C. U. Shepard.*
Coracite . . . . . *J. L. LeConte.*
Corundellite . . . . . *B. Silliman, Jr.*
Corundophilite . . . . . *C. U. Shepard.*
Cryophyllite . . . . . *J. P. Cooke.*
Cryptomorphite . . . . . *H. How.*

Culsageelite . . . . . *J. P. Cooke.*
Cummingtonite (= Hornblende) . . . *C. Dewey.*
Cummingtonite (= Rhodonite) . . . *C. F. Rammelsberg.*
Cuproscheelite . . . . . *J. D. Whitney.*
Cyanolite . . . . . *H. How.*
Cymatolite (= Cumatolite) . . . . . *C. U. Shepard.*
Cyrtolite . . . . . *W. J. Knowlton.*

Danaite . . . . . *A. A. Hayes.*
Danalite . . . . . *J. P. Cooke.*
Danburite . . . . . *C. U. Shepard.*
Daubréelite (*m*) . . . . . *J. L. Smith.*
Dawsonite . . . . . *B. J. Harrington.*
Delawarite . . . . . *I. Lea.*
Deweylite . . . . . *E. Emmons.*
Diabantite . . . . . *G. W. Hawes.*
Dickinsonite . . . . . *G. J. Brush* and *E. S. Dana.*
Dimagnetite . . . . . *C. U. Shepard.*
Ducktownite . . . . . *C. U. Shepard.*
Dudleyite . . . . . *F. A. Genth.*
Dysyntribite . . . . . *C. U. Shepard.*
Dysluite . . . . . *W. H. Keating.*
Dyssnite . . . . . *F. v. Kobell.*

Edenite . . . . . *E. F. Glocker.*
Edwardsite . . . . . *C. U. Shepard.*
Emerald Nickel . . . . . *B. Silliman, Jr.*
Emerite . . . . . *C. U. Shepard.*
Emmonite . . . . . *T. Thomson.*
Enceladite (= Warwickite, *Shepard*) . *T. S. Hunt.*
Eosphorite . . . . . *G. J. Brush* and *E. S. Dana.*
Eremite . . . . . *C. U. Shepard.*
Erilite . . . . . *H. C. Lewis.*
Erusibite . . . . . *C. U. Shepard.*
Erythrite . . . . . *T. Thomson.*
Euchlorite . . . . . *C. U. Shepard.*
Eucryptite . . . . . *G. J. Brush* and *E. S. Dana.*
Eumanite . . . . . *C. U. Shepard.*
Euphyllite . . . . . *B. Silliman, Jr.*
Eupyrchroite . . . . . *E. Emmons.*

Fairfieldite . . . . . *G. J. Brush* and *E. S. Dana.*
Ferroilmenite . . . . . *R. Hermann.*
Ferrotellurite . . . . . *F. A. Genth.*
Ferrotitanite (= Schorlomite) . . . *J. D. Whitney.*
Fillowite . . . . . *G. J. Brush* and *E. S. Dana.*
Fowlerite . . . . . *C. U. Shepard.*
Franklinite . . . . . *P. Berthier.*

Genthite (= Nickel-gymnite, *Genth*) . *J. D. Dana.*
Gibbsite . . . . . *J. Torrey.*
Glossecollite . . . . . *C. U. Shepard.*
Goshenite . . . . . *C. U. Shepard.*
Grahamite . . . . . *H. Wurtz.*
Gunnisonite . . . . . *F. W. Clarke* and *N. W. Perry*
Gymnite . . . . . *T. Thomson.*

Haddamite . . . . . *C. U. Shepard.*
Hallite . . . . . *A. R. Leeds.*
Harrisite . . . . . *C. U. Shepard.*
Hatchettolite . . . . . *J. L. Smith.*
Haydenite . . . . . *P. Cleaveland.*
Hebronite . . . . . *F. v. Kobell.*
Henryite . . . . . *F. M. Endlich.*
Hermannite . . . . . *A. Kenngott.*
Hermannolite . . . . . *C. U. Shepard.*
Hetærolite . . . . . *G. E. Moore.*
Hexagonite . . . . . *E. Goldsmith.*
Hiddenite . . . . . *J. L. Smith.*
Hitchcockite . . . . . *C. U. Shepard.*
Holmesite (= Holmite)
Holmite (= Seybertite) . . . . . *T. Thomson.*
Hortonite (= altered pyroxene). . . .
Hortonolite . . . . . *G. J. Brush.*
Houghite . . . . . *C. U. Shepard.*
Howlite . . . . . *J. D. Dana.*
Hudsonite . . . . . *L. C. Beck.*
Huebnerite . . . . . *E. Riotte.*
Huntilite . . . . . *H. Wurtz.*
Huronite . . . . . *T. Thomson.*
Hydrocuprite . . . . . *F. A. Genth.*
Hydrofranklinite . . . . . *W. T. Roepper.*
Hydromagnesite (pulverulent mineral from Hoboken, N. J., = magnesia-alba, *T. Wachtmeister*) . . . . . *F. v. Kobell.*
Hydroniccite . . . . . *C. U. Shepard.*
Hydronickelmagnesite . . . . . *C. U. Shepard.*
Hydrotitanite . . . . . *G. A. König.*

Ilesite . . . . . *A. F. Wuensch.*
Indianaite . . . . . *E. T. Cox.*
Iodolite (*m*) . . . . . *C. U. Shepard.*
Ionite . . . . . *S. Purnell.*

Jacksonite (= Anhydrous Prehnite) . *J. D. Whitney.*
Jefferisite . . . . . *G. J. Brush.*

Jeffersonite . . . . . *L. Vanuxem* and *W. H. Keating.*
Jenkinsite . . . . . *C. U. Shepard.*

Keatingine . . . . . *C. U. Shepard.*
Kerrite . . . . . *F. A. Genth.*
Küstelite . . . . . *A. Breithaupt.*

Labradorite (= Labradorstein, *Werner*) *J. C. Delamétherie.*
Lancasterite . . . . . *B. Silliman, Jr.*
Latrobite . . . . . *H. J. Brooke.*
Lawrenceite (*m*) . . . . . *A. Daubrée.*
Ledererite . . . . . *C. T. Jackson.*
Lederite . . . . . *C. U. Shepard.*
Leidyite . . . . . *G. A. König.*
Lennilite . . . . . *I. Lea.*
Lernilite (wrong orthography for Lennilite) . . . . . *A. Schrauf.*
Lesleyite . . . . . *I. Lea.*
Leucanterite . . . . . *C. U. Shepard.*
Leucaugite . . . . . *J. D. Dana.*
Lincolnite . . . . . *E. Hitchcock.*
Lintonite . . . . . *S. F. Peckham* and *C. W. Hall.*
Lionite . . . . . *T. Berdell.*
Lithiophilite . . . . . *G. J. Brush* and *E. S. Dana.*
Loganite . . . . . *T. S. Hunt.*
Louisite . . . . . *D. Honeymann.*
Loxoclase . . . . . *A. Breithaupt.*
Luckite . . . . . *A. Carnot.*

Maconite . . . . . *F. A. Genth.*
Macfarlanite . . . . . *A. H. Sibley.*
Maclurite (= Chondrodite) . . . . . *H. Seybert.*
Maclurite (= Pyroxene) . . . . . *T. Nuttall.*
Magnesia, native (= Brucite, *Beudant*) *A. Bruce.*
Magnolite . . . . . *F. A. Genth.*
Mallardite . . . . . *A. Carnot.*
Mangan-amphibole . . . . . *R. Hermann.*
Manganese, ferro-silicate, ... *T. Thomson.*
Manganese, sesqui-silicate (= Dyssnite, *v. Kobell*) . . . . . *T. Thomson.*
Marasmolite . . . . . *C. U. Shepard.*
Marcylite . . . . . *C. U. Shepard.*
Marionite . . . . . *W. Elderhorst.*
Mariposite . . . . . *B. Silliman, Jr.*
Marmolite . . . . . *T. Nuttall.*
Masonite . . . . . *C. T. Jackson.*
Melaconite . . . . . *J. D. Dana.*

Melan-Asphalt (= Albertite, *Robb*) . . *C. M. Wetherill.*
Melanolite . . . . . *H. Wurtz.*
Melanosiderite . . . . . *J. P. Cooke.*
Metacinnabarite . . . . . *G. E. Moore.*
Microlite . . . . . *C. U. Shepard.*
Molybdate of Iron . . . . . *D. D. Owen.*
Monrolite . . . . . *B. Silliman, Jr.*
Montanite . . . . . *F. A. Genth.*
Moronolite . . . . . *C. U. Shepard.*
Mullicite . . . . . *T. Thomson.*

Nacrite (= Muscovite) . . . . . *T. Thomson.*
Necronite . . . . . *H. H. Hayden.*
Nemalite . . . . . *T. Nuttall.*
Neurolite . . . . . *T. Thomson.*
Newportite . . . . . *J. G. Totten.*
Niccochromite . . . . . *C. U. Shepard.*
Nickel-gymnite (= Genthite, *Dana*) . *F. A. Genth.*
Nickel vitriol . . . . . *T. S. Hunt.*
Nuttallite . . . . . *H. J. Brooke.*

Ontariolite . . . . . *C. U. Shepard.*
Owenite . . . . . *F. A. Genth.*
Ozarkite . . . . . *C. U. Shepard.*

Paracolumbite . . . . . *C. U. Shepard.*
Parailmenite . . . . . *C. U. Shepard.*
Parathorite . . . . . *C. U. Shepard.*
Parophite . . . . . *T. S. Hunt.*
Pattersonite . . . . . *I. Lea.*
Paulite . . . . . *A. G. Werner.*
Peckhamite (*m*) . . . . . *J. L. Smith.*
Pelhamine . . . . . *C. U. Shepard.*
Pennite . . . . . *R. Hermann.*
Peristerite . . . . . *T. Thomson.*
Perthite . . . . . *T. Thomson.*
Philadelphite . . . . . *H. C. Lewis.*
Phlogopite . . . . . *A. Breithaupt.*
Phosphuranylite . . . . . *F. A. Genth.*
Phyllite . . . . . *T. Thomson.*
Phytocollite . . . . . *H. C. Lewis.*
Polyadelphite . . . . . *T. Thomson.*
Polylite . . . . . *T. Thomson.*
Prehnite, anhydrous, ... *J. D. Whitney.*
Priceite . . . . . *B. Silliman, Jr.*
Protovermiculite . . . . . *G. A. König.*
Psittacinite . . . . . *F. A. Genth.*

Pyromelane . . . . . *C. U. Shepard.*
Pyrophosphorite . . . . . *C. U. Shepard, Jr.*

Rahtite . . . . . *C. U. Shepard.*
Randite . . . . . *G. A. König.*
Raphilite . . . . . *T. Thomson.*
Rastolyte . . . . . *C. U. Shepard.*
Reddingite . . . . . *G. J. Brush* and *E. S. Dana.*
Remingtonite . . . . . *J. C. Booth.*
Rensselærite . . . . . *E. Emmons.*
Retinalite . . . . . *T. Thomson.*
Rhodophyllite . . . . . *F. A. Genth.*
Richmondite . . . . . *A. Kenngott.*
Rœpperite (= iron-manganese chrysolite, *Rœpper*, = Stirlingite, *Kenngott*) . *G. J. Brush.*
Rœpperite (= iron-manganese dolomite) *A. Kenngott.*
Rogersite . . . . . *J. L. Smith.*
Roscoelite . . . . . *J. Blake.*
Rutherfordite . . . . . *C. U. Shepard.*

Schirmerite . . . . . *F. M. Endlich.*
Schirmerite . . . . . *F. A. Genth.*
Schoharite . . . . . *A. Eaton.*
Schorlomite . . . . . *C. U. Shepard.*
Schreibersite (*m*) . . . . . *C. U. Shepard.*
Seybertite . . . . . *T. G. Clemson.*
Shepardite (= Schreibersite, *Shepard*) (*m*) . . . . . *W. Haidinger.*
Siderophyllite . . . . . *H. C. Lewis.*
Silicoborocalcite (= Howlite, *Dana*) . *H. How.*
Sillimanite . . . . . *G. T. Bowen.*
Sipylite . . . . . *J. W. Mallet.*
Somervillite . . . . . *A. Dufrénoy.*
Sonomaite . . . . . *E. Goldsmith.*
Spartaite . . . . . *A. Breithaupt.*
Spartalite (= Zincite) . . . . . *H. J. Brooke* and *W. H. Miller.*
Steeleite . . . . . *H. How.*
Stellite . . . . . *T. Thomson.*
Sterlingite . . . . . *J. P. Cooke.*
Sterlingite (= Zincite) . . . . . *F. Alger.*
Stibioferrite . . . . . *E. Goldsmith.*
Stirlingite (= Rœpperite, *Brush*) . . *A. Kenngott.*
Sussexite . . . . . *G. J. Brush.*

Talkeisenerz . . . . . *A. Breithaupt.*
Telaspyrine . . . . . *C. U. Shepard.*
Tellurbismuth . . . . . *D. M. Balch.*

Tephroite . . . . . *A. Breithaupt.*
Terenite . . . . . *E. Emmons.*
Texasite (= Emerald Nickel) . . . . *A. Kenngott.*
Thalite . . . . . *D. D. Owen.*
Thinolite . . . . . *C. King.*
Tincalconite . . . . . *C. U. Shepard.*
Torrelite . . . . . *T. Thomson.*
Trautwinite . . . . . *E. Goldsmith.*
Triploidite . . . . . *G. J. Brush* and *E. S. Dana.*
Troostite . . . . . *C. U. Shepard.*
Tungstic Ochre . . . . . *B. Silliman.*
Tysonite . . . . . *O. D. Allen* and *W. J. Comstock.*

Unionite . . . . . *B. Silliman, Jr.*
Uranothorite . . . . . *P. Collier.*

Vanuxemite . . . . . *C. U. Shepard.*
Venerite . . . . . *T. S. Hunt.*
Vermiculite . . . . . *T. H. Webb.*
Vermontite . . . . . *A. Breithaupt.*

Warwickite . . . . . *C. U. Shepard.*
Washingtonite . . . . . *C. U. Shepard.*
Wheelerite . . . . . *O. Loew.*
Whitneyite . . . . . *F. A. Genth.*
Willcoxite . . . . . *F. A. Genth.*
Williamsite . . . . . *C. U. Shepard.*
Wilsonite . . . . . *T. S. Hunt.*

Xanthitane . . . . . *C. U. Shepard.*
Xanthite . . . . . *T. Thomson.*

Youngite (= Hornblende)

Zaratite (= Emerald Nickel) . . . . *A. Casares.*
Zincite (= red oxide of zinc, *A. Bruce*) *W. Haidinger.*
Zinc, silicious oxide, ... *L. Vanuxem* and *W. H. Keating.*
Zircarbite . . . . . *C. U. Shepard.*
Zonochlorite . . . . . *A. E. Foote.*

CONTRIBUTIONS TO THE GEOLOGICAL HISTORY OF THE AMERICAN CONTINENT.

[The Address of the retiring President, delivered before the Montreal Meeting of the American Association for the Advancement of Science, 1857.[1]]

BY

JAMES HALL.

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

ONE who accepts a trust has no right to make excuses for not fulfilling its obligations; and yet I would willingly excuse myself from the performance of this last act of the retiring President.

With imperative duties enough to occupy every hour of my time, I cannot do justice to you, gentlemen, or to your kind partiality in assigning to me this position. Circumstances beyond my control have placed it out of my power to make the preparation which was due to the occasion.

You have so often been reminded of the high objects and purposes which bring us annually together, that I need not repeat this now. It is the one great social and scientific festival of the man of science throughout the year, of all others, the most desired: — to the tired and lonely worker, who is endeavoring to add new truths to our stock of knowledge, or to elucidate more clearly what we but partially know; to the teacher of science who has

[1] It is only proper for the author to say that during this long interval he has taken no opportunity of revising the original manuscript of his address, which he had long hoped to be able to accomplish. It has been copied without any supervision by him or any changes suggested by him. It has been his desire to present the address as then given, and to make any desired changes or explanations by supplementary notes. A few changes in proof correction, in order to render some points more clear, or to avoid repetition are all that have been done. None of the important additions to our knowledge of the subjects discussed, during the past quarter of a century, have been either directly or indirectly referred to or incorporated in the address.

The address is submitted to the scientific public as an expression of the views and opinions entertained by the writer at that period.

been freed from his daily routine of duties;—this occasion is one of many pleasant anticipations. We all meet as one brotherhood, as the children of one Parent under whose government we delight to live, and whose laws, impressed upon ourselves and upon everything around us, we would better understand. We come together for mutual counsel and mutual aid for the true interpretation of these laws respecting our relation to the animate and inanimate world around us, and our relations to the Great Author of the whole.

On every recurring occasion of our meeting it is a subject of congratulation; and more particularly for those who can so well remember when the year wore on without those pleasant anticipations of a reunion with sympathizing friends and colaborers in science. But this occasion, more than all others, gives cause for congratulation,—cause even to excite enthusiasm, that true to the enlightened views we profess, and true to the instincts of an unprejudiced scientific spirit, we have forgotten political and civil limits, have forgotten the names of governments and countries, and with cosmopolitan spirit we meet beneath the ægis of Her Britannic Majesty, Queen Victoria, though still upon the American Continent; and now may we more fully claim the right to the title of the AMERICAN Association for the Advancement of Science.

I might, were it the proper occasion, go on to say, in how far the establishment of telegraph wires, railroad lines and various social and commercial relations have tended to bring about this meeting in Canada; but scientific sympathies have done more; and I cannot help thinking too, that the discovery of another fact, that the rocks are of the same age and the same character in Canada as in the adjoining territory, has, in a manner served to assimilate the living inhabitants of this wide country, by showing most conclusively that the ancient inhabitants of this continent were of one nation and one government, and indeed are here to-day with us, if not to discuss, at least to furnish subjects of discussion, and who will maintain their ground notwithstanding geological systems and system makers. Nor should it be forgotten that the geologist of Canada, with that noble and unselfish spirit which characterizes the true man, and the man of real science, has adopted a nomenclature already framed for an adjoining State; for although directing the investigations over an area six times as large as New York, he saw nothing in the prolongations of the

same beds that could induce him to change the name of rocks already well determined. With this immense area Sir William Logan had a right to establish his own nomenclature; but in true scientific magnanimity, which affects no local nomenclature, Canada has been added to New York and the West, that over our common country may extend a unity of scientific language.

But from the present, which brings only delightful associations, shall we not turn one moment to the past, and to the memories of the departed? The last year has been a sad one for our little band of workers in science. Never has a year before cut down so many true and tried ones from amongst us. The names of Redfield, Tuomey, Mitchell and Bailey, need but be mentioned to find a mournful response in the heart of every one who knew them.[2]

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Our association, composed as it is of laborers in every department of science, has perhaps a right to expect that the retiring President should embrace in his discourse a view of the progress of science in America for the past year. But even if the investigator in one science were able to present such a view, I do not believe that the time allotted for our annual address is sufficient for such a retrospect.

For one who is in constant intercourse with the mute and inanimate existences of former creations, there is little hope that he can offer a language that shall be acceptable to the living, or that his subjects shall interest others than those who are themselves engaged in the same pursuits; and yet, gentlemen, in my poverty, I have nothing else to offer you than the results which flow from geological and palæontological investigations during the past few years, and to offer collaterally some *Contributions to the Geological History of the North American Continent.*

While I would claim for no science a preëminence, I must here protest against that narrow view which has been expressed by some, that Natural History is not a science, and who perhaps believe that mathematical formulæ lie at the basis of all sciences and that these must be used in all their minutiæ to reach any valuable result. But let us inquire whether, after all, natural science

[2] The long interval of time which has elapsed renders it undesirable to repeat here what immediately follows concluding the introductory portion of the address.

as it is termed by way of distinction (one might suppose to distinguish it from *artificial* science) be not truly a mathematical science. Every object in nature is a mathematical figure of some sort, and its form and proportions may be expressed in mathematical formulæ.

Let us take one of those little bodies known as crinoids, which are of various spheroidal or ovoid shapes. They are made up of plates of various forms, three, five or seven sided, with sides of various length and angles as various. More than this, these plates are not plane surfaces, and their curvature has such relation to the sides and angles, and to the form of the whole body, that when put together they shall produce just that form and no other. And now will you tell me by what processes a mathematician will set about calculating the length of the sides, the proportions of angles and the requisite curvature of each of these plates? Long before the mathematician could arrange his formulæ, the naturalist has instinctively measured all, and has reconstructed the body as unerringly as the motion of light.

But then you will ask me, perhaps, what is the use of all this? I will tell you of a use for it. When, like the navigator at sea, the naturalist may be wandering many hundreds of miles away from any fixed point in the geological horizon or geological sequence, he may meet with some dismembered plates of one of these bodies in the rock upon which he stands—no mathematical calculations, no astronomical observations or chemical analysis, will serve to determine the geological latitude and longitude. The density of the mass fails you, its chemical composition will not aid you, and you are thrown back upon the few poor pentagonal, hexagonal or heptagonal plates of a little insignificant body by which to determine the place. Do you measure the sides and angles and sit down to work out the formula with your known and your unknown quantities represented by initial characters? No! The naturalist instinctively, with mathematical exactness, sees the elements of the problem, and the result itself stands revealed before him. All that every other science has failed to do is here accomplished.

I might even occupy the entire time of this address in explaining the beautiful and curious structures of the crinoids. In some periods of geological history they are, so far as known, few, and their mode of construction is in each type very distinct, and every

plate is recognizable not only as referrible to the species, but to its place in the body.

In later periods of the palæozoic era there are great numbers of species constructed upon the same general plan, the same number and form of plates at the base, and in the first, second and third series or ranges of plates of the body; and although showing distinctions in the different species, they are not so strongly marked because of the great number of individuals constructed upon the same general plan. But as if in order to individualize themselves, and to overcome the want of character arising from similarity in basal structure, they have adopted another mode of distinction which is equally dependent on mathematical principles. The bodies are surrounded near the summit by arms, and there is a bilateral arrangement so that when viewed from one side which is called the oral side, the two halves correspond. Now on each side of this dividing line you will find the arms originating in one instance in pairs or double, and this prevails throughout, and you have ten arms in five pairs, *et cetera*. The first deviation from this is a group of *three* arms on each side of the ovarian aperture, while all the others are in pairs. Again you have groups of four on either side of the ovarian aperture, and groups of four in double pairs throughout the circumference. They present every possible combination of the numbers 2, 3, 4 and 5, in this manner individualizing themselves where the type is multiplied in species; and these are among the lowest forms which engage the attention of the palæontologist.

Without showing how, at every step from the lowest coral cell to the higher forms of life, the naturalist becomes the mathematician, it will be enough to mention to you the achievement of a naturalist (whose name I need not repeat here) in the conception and production of the outline of a fish from a single scale,—the solution of a problem upon purely mathematical principles aided by a profound knowledge of structural development.

Could the physicist have ever determined the original areas of land and water upon the face of the globe during former geologic periods, or have proved that the sea, once spread over continental areas, was always shallow during many successive creations?

Even when we had determined that our continent was outspread with the sediments of a former ocean, could any geometer show us the direction of the ancient current like our modern Gulf

Stream or its lines of greatest force and the lateral subsidence of this force? and could he point out the quiet mid-ocean areas that prevailed during that long, long palæozoic period, or during later geological ages?

Geology, then, may assume to know something of the structure of the earth, not less than the sciences which weigh and measure it; and when the vast accumulations of geological collections in our country shall be reduced to system, and the story of their origin told, we shall learn that there are laws in operation not differing from other natural laws, though seen and studied by different modes.

The picking up of stones by a child may not be science, nor is the blowing of soap bubbles by the same child any more a science, but either one of these amusements by a philosopher may lead to the discovery of laws which we call science.

It is here that one of the great difficulties of geological science lies; for geology presents herself to us in two aspects, the Physical and the Natural History. I might add also as a third, the Chemical aspect, so large a part has chemistry played in geological phenomena, being indeed at the foundation of many physical changes, and because there are no higher problems on which chemical science has expended her force than those furnished by geology.

To some minds, geology scarcely assumes the rank of a science, except where treated of from a physical point of view. They consider the simplest physical law adequate to the explanation of the most stupendous phenomena. To them, mountain chains rise and are abraded, and the entire crust of the earth is folded and plicated in obedience to certain laws. They see no difficulty in the way of imagining torrents of water moving onward and upward, carrying masses of rocks over heights far above their origin by some simple gyratory force. The entire earth becomes with equal ease to them, either a pliable, elastic, or compressible mass, or a non-elastic body; sometimes one, sometimes the other, changing gradually, or by sudden cataclysms, according to the fancy of the expounder. No wizard's wand ever played so many pranks as the poor earth in the hands of these theorizing geologists.

The more slowly moving, but not less industrious and devoted laborers in science, can never hope to climb the height of that Parnassus which the physical theorist in geology reaches at a single bound. We stand almost aghast at the passage and the

progress, but after all we are forced to confess that our *wonder* has been more excited than our reasoning powers; and I am in duty bound to say that geology has suffered more from these theories than from all other causes, though we have much to fear from the natural historians at the present time.

It is the misfortune of all sciences that when, having acquired a certain degree of popularity or interest in the public mind, men are found who, with more or less of true science, are still so fond of making theories of their own, that they at once invent a plausible and acceptable plan which goes forth to the world with all the circumstance attending a real scientific investigation. With truth enough to spare it from the anathemas of science, it still carries false doctrines and false views. Nor is this peculiar to geology, for other sciences have suffered equally from the false zeal of injudicious friends who attempt the reconciliation of new and legitimate views of science with old prejudices and preconceived opinions.

In geology this reconciliation is attempted between the science and Scripture, and every phase of geological science has had its reconciliation with the received opinions or prejudices regarding the origin of our earth, as derived from the first chapter of Genesis. The absurdity of all this is apparent, when we consider that with advancing discoveries every quarter of a century presents our science in so different a phase that a *new reconciliation* is required. Yet each reconciliation and interpretation is advanced with *ex cathedra* authority; and not satisfied with promulgating scientific truths, the author would insist upon making their interpretations dogmas of religious teaching and religious belief.

Another point which I conceive to be more pernicious than this is the Millerism in geology at the present time. But time will not permit a review of this subject.

Let me now, gentlemen, attempt to present you with something more peculiarly appropriate to this occasion; and, not to go beyond the pale of geological science, I may be permitted to call to your attention some of the more important results which have been derived from the investigations of the past few years.

There was a time when we were taught to believe that all the mountain regions of New England, the Appalachian chain, those of Northern New York and the Laurentian mountains, were pro-

jecting prominences of the ancient, irregular, primary nucleus of this portion of our planet; but that time has long since passed, and I conceive it to be one of the most important steps in the progress of modern geology that, having shown most conclusively that this science has nothing to do with the origin of things, we have proved that she recognizes no *beginning* of existing conditions. There is no *known* point that can be appealed to as a part of the primary nucleus of the earth, according to the theory which regards the earth as formerly an incandescent body.

All that we can see among the evidences of the earlier condition of the globe shows us that the physical conditions were then as now: that even the *beginning* which we recognize in geology shows our continent as an ocean-bed laved by the widespread waters, and solid materials broken down and transported then as now by currents and spread out in stratified deposits over the ocean-bed. The evidences of these conditions meet us in the elevated ranges of the Adirondack mountains in New York and the Laurentian mountains of Canada, and various isolated points elsewhere. These most ancient of all the rocks which we know tell us only of *water* and its distributing action in first spreading abroad the materials, and their subsequent metamorphism and elevation.

How many similar sedimentary formations may have preceded this one, we know not, but we have reason to believe that this is not the earliest, and we may yet know more of those of prior date. And even now the investigations in the Canadian Survey have shown the necessity of dividing the sediments of this period which we have considered as one; the facts proving these rocks to have been formed during two epochs, forming the Lower and Upper Laurentian.

We have moreover the evidence of preëxisting stratified deposits, for here in these ancient sediments of the Laurentian period are found enclosed masses of preëxisting stratified rocks. Even these ancient crystalline masses, which had been believed to be the oldest if not the very primordial nucleus of the earth, are found to contain laminated masses, not conforming to the stratification or lamination of these beds, but lying at a different angle, telling you that here is a fragment of some *older* stratified mass, which had been broken off, transported by the ocean current, and embedded among the finer débris of a later deposit. Even here then we find

that not only are these rocks not the primary nucleus, but that the materials of which they are made are not derived directly from that nucleus.

Instead therefore of the earlier condition of our planet exhibiting the action of great heat, and the results of igneous influences, we find only evidences of water, for metamorphism does not necessarily imply the action of heat in a high degree. Here are the oldest rocks known, still preserving their lines of bedding and their outcrops, as distinctly marked as in the more modern formations.

We are indebted to the Geological Survey of Canada for fully establishing this *Laurentian System* as a series of older stratified deposits and the promulgation of another, the *Huronian System*, which must hereafter form a part of the geological history of our continent.

These Laurentian deposits of immense thickness, and regularly stratified, are nevertheless almost throughout highly crystalline. In the gneiss of this period, which preserves its regular lamination, were embedded large masses of a preëxisting gneiss, with its planes of lamination lying at various angles to the lines of bedding in the enclosing rock. Masses of this kind often appear like an immense accumulation of bowlders, the interstices of which have been filled with sand and mud, the whole having become indurated and subsequently metamorphosed; the bowlders retaining the character and lamination of their original bedding, while the new deposit has assumed its own system of lamination conformable to the lines of bedding.

In this manner we have disposed of these areas of crystalline rocks belonging to the more ancient class, or those prior to any evidence we have of the appearance of life upon our planet; but the analogy in their mode of occurrence, and consequently their manner of formation with the fossiliferous deposits, induce the hope, and I may say even the expectation, that we shall one day prove them to have been the seat of animal and vegetable life. Notwithstanding that our experience thus far negatives such inference, I hold that there is nothing unphilosophical in the belief that all the conditions of their deposition, so far as we now can see, were as favorable to life as the newer formations, and there is no ground upon which we can safely conclude that animal remains will not yet be found in these rocks. That there was a period

when life began upon our earth I do not doubt, but there are no evidences beyond those of a negative character, to prove that it might not have begun long previous to the commencement of those rocks at present known as fossiliferous. This same negative evidence has been applied to New England and other portions of the country long after many of us had shown that the mountains of New England consisted of strata of fossiliferous age; and it will probably be a long time yet before all will admit that the Green mountains are Silurian, and the White mountains Devonian and Carboniferous, in their age.

In one respect the lower crystalline rocks present a point of high interest. In their greatest development they are known only towards the north, both in America and Europe. On this continent they rise to the northeast, forming a long barrier nearly parallel to the line of the St. Lawrence; and thence stretching away to the westward by the great lakes they gradually die out in low isolated areas, which form no strong feature, and are finally entirely obscured by later formations. It may still be said that we have intrusive granites of older date, and that these may be parts of that original nucleus. There is no evidence to sustain this view. Granitic masses penetrating the Laurentian rocks may still be portions of the preëxisting stratified deposits; and when we find crystalline granite intruded in the more recent Silurian, Devonian or Carboniferous strata, we may infer that they are derived from the formation entirely beneath. We have no evidence that, even by granitic veins or extruded masses of so-called igneous granite, there is any communication from the surface with that imagined primary incandescent nucleus of our earth.

This point prepares us for a different appreciation of the phenomena presented by crystalline masses. If it does not simplify the whole subject of geological science, it at least takes away the source whence have been derived a large part of those absurd speculations, where imaginary laws of physics and chemistry have been applied to illustrate and sustain an equally false and imaginary geology.

The grand problem of geology is the entire history, chemical, physical, zoölogical and botanical, of the groups of strata constituting the formations of the globe. From the first evidences of life as we know it in the strata, to the present time, is the phase in the earth's history with which geology has most to do; and it

is here that the labor of systematizing and classifying has been mainly expended. But even with the best intentions and with the best efforts there are different views expressed, and one type or another, as seems most in accordance with the author's views or his knowledge, is made to mark the eras or periods. This is true not only of the more general but also of the more detailed classifications which have been advanced and adopted.

All these classifications must, confessedly, be founded upon imperfect knowledge, for the geology of the globe has been but partially studied, and negative knowledge, or the absence of all knowledge, is too often treated in generalizations as positive knowledge.

It might be presumptuous to hope to advance any new views upon this subject; still there is one which seems to me not likely to receive sufficient attention in the increasing interest and attractions of palæontology. This science instead of being studied in connection with geology, and as an exponent of geological phenomena, has come to be regarded as a great object of independent investigation; and rocks are looked upon merely as the conservatory or museum of the natural history of the past geological periods. Certainly I ought not to oppose a view which exalts palæontological science; and still, gentlemen, I would not in this study lose sight of the attendant physical conditions, which are equally important to be regarded. The progressive steps in the development of animal life have not been made without great physical changes; and we can never arrive at a true history of our planet till both are studied in connection.

The relation of the physical conditions to the vital force and to its development is a problem of vast importance. It should always be borne in mind, that the expression of animal life is coincident with physical conditions, and that the vital force is never developed independent of physical conditions. This is true without giving any undue importance to these conditions, or any countenance to materialistic views.

The subdivisions into formations, groups and systems, have always been marked by physical changes. Where a group of strata has its materials like those of the preceding group, there will the forms most resemble those of the preceding period; and where the lithological change is greatest, there will be found the greatest change in the nature of the organisms. When an intervening

formation cuts off all the species of a preceding group, and the next, or third, is similar to the first in lithological character, so far will the fossils resemble each other not only in generic forms, but in the characteristics of species. We have then the converse of this proposition, that uniform physical conditions give uniform fauna. And I may here say, that while this holds in a large majority of instances, it does not hold true in all; because there is a law still beyond that of physical condition which affects the geographical distribution of species. In modern zoölogy, we have geographical centres of distribution; and we have zoölogical districts or areas both on land and in the sea. Here questions are involved which have not been heretofore fully discussed; but which nevertheless — so long as each geological period is marked by a fauna of its own, and is compared with a modern fauna — demand our serious consideration.

In pursuing this subject, let us for a moment review the recognized periods of American geology, or the geological systems and groups as at present accepted, and their application to the rocks as developed upon our continent. With true scientific spirit, as well as in respect to what has been done on the continent of Europe and in Great Britain, we endeavor to make the greater groups or systems harmonize with those adopted in Europe, while all the minor divisions must be wrought out and receive the expression due to them from the conditions of this continent, dependent upon the prior conditions of the great ocean in which they were deposited. In tracing the development and distribution in that part of our continent which we intimately know, I shall be able to show with more force the results at which I have arrived respecting certain *mountain* and *continental phenomena.*

The Potsdam sandstone, in its wide distribution, is everywhere marked by ripple marks, by broken and drifted shells of *Lingula*, diagonal lamination and all the evidences of a shallow sea. In the uniformity of the character of its materials and its even distribution over wide areas, it is only equalled by its associated member the Calciferous sandstone or Magnesian limestone which, in composition and the uniformity of physical conditions under which it was deposited, presents similar characters. Even long prior to the period of these depositions, the forces which had disturbed the preëxisting continent had ceased, and in contradiction to the generally entertained opinion that in more ancient times

there was more disturbance of the crust, there has scarcely ever been, since this advent of life upon our globe, so wide an area or so long a period, indicating repose of all the disturbing forces. At no period has deposition been so uniformly diffused or animal life so widely distributed in the same forms over so large a proportion of our continent.

The second great period of the geological history is marked by the limestone groups, the Chazy, Birdseye, Black River and Trenton. The latter is the most important and gives character to the whole. This formation we know to be widely spread over the continent, indicating until near its close a period of repose, and it is only in some few points towards the northwest that we discover the deposit much attenuated and the influx of muddy sediments interfering in a great degree with the persistence of the fauna.

I might state here incidentally that the fauna of the Potsdam and Calciferous period is totally unlike that of the Trenton limestone period following. In the earliest of known periods when life has flourished, it has been widely distributed and has ceased without convulsions or cataclysms, leaving the area of ocean which it had occupied for the occupancy of new races. In the very first of our creations, therefore, we have evidence to show that life was terminated over wide areas while there are no evidences remaining of violent commotions.

The next succeeding or the Hudson River group appears to us with more features of interest in its physical characters than either of the preceding; and it was at this period, as I shall attempt to show, that the form and outline of our present continent were determined.

The Hudson River group presents us on the one hand with a series of soft shales becoming coarser and alternating with sandstones above, and on the other with irregular masses of limestone and finally immense masses of coarse sandstones or conglomerates with great bands of shale. This group attains in Canada a thickness of 2000 feet, and in Pennsylvania it has a thickness, according to Prof. Rogers, of 6000 feet. Its northeastern extremity stretches out to Gaspé, and on the south side of the St. Lawrence, and, throughout Canada and the Green mountains of Vermont, forms in its metamorphic and folded condition, a great feature of the country. In its continuation southward it forms a marked feature in the Appalachian chain gradually dying out as the range declines

to the southward. Along this line, which I shall term its line of trend, it presents no striking variations—nothing but what might be expected in any sedimentary group of rocks. It is only in pursuing it to the westward that we find interesting and important changes. Tracing it through Canada, west by the islands of Lake Huron and by Green Bay to the Mississippi River and more southerly still by Cincinnati and the mouth of the Ohio, we learn that the coarser sediments gradually disappear, the finer mud alone having been transported, and finally that calcareous matter becomes an important ingredient in the formation and sandstone has become an exception. Or to give the best expression to the change, we here in New York and Canada speak of the shales and sandstones of the Hudson River group terminated by the Oneida or Shawangunk conglomerate. In Ohio the same group is known as the "Blue limestone." In Wisconsin and Illinois, this group, which in New York and the east is thousands of feet in thickness, has thinned so that it measures only from sixty to one hundred feet, while in Iowa I have not been able to obtain a thickness of fifty feet. Here we have an extensive group of strata changing from fine and coarse sedimentary materials and passing through all the intermediate phases until it becomes a shaly limestone.

Now in looking at the present continent as at that time an open sea, I conceive that the most rational explanation of these phenomena is that a powerful current has brought these sedimentary materials of sand, clay and pebbles from the northeast[3] and has distributed them along the line now marked by the Appalachian chain; or, if you please to assume that there has been a continent to the east of our own which has been subsequently submerged, then the materials have been distributed along its coast. On whatever ground we view the deposit, we must admit, I conceive, that these materials have been distributed by a current, and that the direction of this current has been in the line of greatest accumulation and coarser materials; and that towards the westward, where we find less accumulation and finer materials, but still a wide ocean, the current gradually diminished until it essentially ceased and the fine materials were slowly spread over the broad area which they occupy in their diminished thickness. Such I conceive to have been the physical conditions of this an-

[3] See note at the end of address.

cient ocean-bed, an ocean disturbed by currents as extensive and as powerful as any we can point to in modern oceans. Thus from an equable and comparatively quiet period when the Potsdam sandstone was spread widely over the bottom of the sea, we have come to one when powerful currents directed the distribution of the sediments in long lines of deposition, on the seaward side of which we find the gradual thinning-out of the beds for want of a sufficient transporting agency.

Shall we here inquire, what influence this change in the character of sediments has had upon the fauna of the period; and if the fauna in any manner indicates the force of physical influences? In the east the Hudson River group presents an abrupt change from limestones to black and green shales containing few fossils. In the sandstones above and in the calcareous bands accompanying them are numerous plants, graptolites, some crinoids and acephalons, with gasteropods and cephalopods not known in the group below; while at the same time after being absent through the lower part of the group, many of the Trenton limestone species of brachiopods are here reintroduced midway in the series mingled with the new fauna.

In the west where the strata are calcareous and have a lithological similarity with the formation below we find, likewise, a greater similarity with the fossils of the preceding period. Nearly all the Trenton brachiopoda are found, together with a large proportion of new forms of this family. The acephalous shells, which are the distinguishing fossils of this group in New York and part of Canada, are rare and very subordinate in the west, forming indeed no notable portion of the fauna. At the same time the trilobites of the Trenton limestone occur abundantly in the Hudson River group of the west while they are extremely rare in its coarser sedimentary phase in the east. But even these phases do not hold good throughout, and in western Wisconsin, northern Illinois, and in Iowa towards the attenuated margins of the group, scarcely a single species of the group is identical either with those of Ohio or of New York or Canada; and, finally, on its attenuated northwestern margin it is marked by scarcely more than half a dozen species of fossils, not one of which could be recognized as identical with anything known in the group within two hundred miles from the centre of its greatest development. Such are the phases of

the Hudson River group in the great eastern development and in its western and southwestern prolongations, and this was the extent of our knowledge of its phases until within a year past [1856]. During this time the investigations of the Canadian survey under the direction of Sir W. E. Logan have extended to the Island of Anticosti, hitherto almost a *terra incognita*, and the few fossils we had seen from there indicating very clearly a new and rich field. But the results are far in advance of all anticipation.

In order, however, to understand fully these results and the position of that Island, you must know that from this point to below Quebec and still farther down the St. Lawrence, the lower formations with the Hudson River group in its vastly developed physical aspect, together with the formations below, abut against the slopes of the Laurentian mountains; that to the eastward the trend of the Hudson River in its conglomerates, etc., is more easterly while the Laurentians trending to the northeast open a space between them in which lie the Island of Anticosti and the Mingan Islands. We might have expected to find on the Island of Anticosti the upper sedimentary deposits of the Hudson River group with some higher beds, but we were quite unprepared for the result which shows that here, as at the west, are large accumulations of calcareous beds with the recurrence of fossils similar to those along the area exposed by the Cincinnati axis. The same corals, the same brachiopods that mark the "blue limestone" formation of Ohio and Indiana and of the islands of Lake Huron are, here in Anticosti, characteristic forms. On the west the nearest points where these associations of fossils are known are the islands of Lake Huron, three hundred miles or more to the westward of where we stand; while on the other hand, seven hundred miles or more to the eastward, are the same associations of fossils. In Anticosti there is the well-marked coral *Favistella stellata* with *Orthis subquadrata*, marking the same horizon as one thousand miles farther to the west, while all the intermediate space is filled with the sedimentary deposits of the group, in which neither of these forms occurs. To the southward of Anticosti, there has been the great force of that current from the northeast bringing forward the sediments of the Hudson River group, while the more gentle action spread northward the finer material, to be mingled with the calcareous matter there formed from corals and shells and to become

the habitation of another fauna. And not only do we find there many species which we know as characteristic of western localities, but many other similar forms both of corals, brachiopoda and gasteropoda, besides many new and remarkable forms. Instead therefore of this group of brachiopoda and other fossils forming, as we had supposed, a peculiar fauna marking the western extension of the Hudson River group, it has, in truth, its centre farther to the east, and the western apparition is an isolated offshoot, as it were, which after all has had but a meagre development when compared with that of Anticosti. The trend of the coarser materials, striking against the southern prolongation of the Laurentian range, cuts off the western extension of these more quiet sea-deposits which were only again reproduced and continued at the westward and southwestward.

The strata which we designate therefore as our Lower Silurian formations have been deposited under very unequal conditions: in the first instance an almost universal sediment of uniform character, ending with calcareous materials marked by a fauna as peculiar as any other; then an entire cessation of these conditions and a new fauna scarcely less universal in its distribution in the earlier epochs such as the Trenton limestone. In the later phases of the Hudson River group it exhibits evidence of violent physical action in the accumulation of coarser debris, powerful currents and unequal distribution of material, with a varied fauna. Certain parts of the group in its geographical distribution are characterized by fossils which are almost or quite unknown in other parts.

Almost coincident with the distribution of the sediments of the Hudson River group to the west and southwest are those of the Medina sandstone and Clinton groups. These later formations gradually die out in the line of the Great Lakes on the westward and have scarcely a vestige in the Ohio valley; while they are, like the Hudson River group, much more persistent in the direction of the Appalachian chain. Although we can only regard these products as the result of the same action which produced the sediments of the Hudson River group, yet we admit the culmination of that action and of that group in the Oneida conglomerate; while the Medina sandstone and Clinton groups in their sedimentary materials must be regarded as the result of the diminishing force which originated and continued in greater or less degree

throughout the period. At the time the Medina sandstone (and perhaps the Oneida conglomerate) as well as the coarse materials of the Clinton group were being deposited in New York and westward, the sediments of Anticosti were made in a comparatively quiet sea, and the succession of forms goes on almost without interruption. You perceive that there were already at this early period vastly greater accumulations of sediment along what is now the eastern border of our continent than along the country to the westward and especially in that part now constituting the Mississippi valley.

We are now to contemplate a return to a more quiescent condition of the ancient ocean. The shales, sandstones and conglomerates are succeeded by limestone formations of wide extent. The Niagara group, stretching from the Hudson valley westward, can be traced through New York, Canada, by lake Huron and lake Michigan, thence across Wisconsin and Illinois, where it disappears beneath the Devonian and Carboniferous rocks of Iowa and Missouri. In its southwesterly direction from New York it gradually thins out, but is still known in Tennessee where it is lost beneath the more recent formations. The numerous fossils in the Canada Survey collections proclaim the existence of this group in the peninsula of Gaspé and at numerous other points on the northeast.

Without at this moment giving the deductions which arise from the facts, I may mention here that the Niagara group and succeeding limestones lie uncomformably upon the Hudson River group, in the valley of the Hudson; that, even before the deposition of this member of the series, those contractions which have finally produced the results of which we have been before informed, in the Green Mountain and Appalachian chain generally, had at this early period commenced.

The Medina sandstone and Clinton groups, which lie at the base of the Upper Silurian System and below the Niagara group, have had but a partial distribution of sediments in a westerly direction, while the succeeding limestones of the Niagara are as widely distributed as any Silurian formation upon our continent. Notwithstanding this disturbed condition during a large part of the time of the deposition of the formations at the base of what we term Upper Silurian, a new fauna was introduced, and as we should sup-

pose under most unfavorable circumstances. Short periods of repose indicated by the nature of the strata, alternated with periods of disturbance from the influx of coarse sediments, which must have been disastrous to animal life. We might have presumed that the fauna we find in these rocks was a colony, as it were, from some more favorable centre, but we have not until now had the means of proving that in the same period the island of Anticosti was a quiet sea and there flourished the prototypes of our upper Silurian fauna.

The conditions which existed during the Niagara period, caused by the elevation of the eastern portion of the Hudson River group, thus producing a thinning of the easterly edge of the Niagara group, continued after the cessation of its fauna. Beginning with its thin edge in eastern New York it becomes conspicuous in western New York and Canada West and is thence recognized by the islands around the northern margins of the Great Lakes and becomes an important formation at the Mississippi River. Thus without any apparent disturbance and with a continuance of the same physical conditions which continued to cause the deposition of the same kind of sediments, we have a total change in the fauna marking this later stage, as it were, of the Niagara period. The conditions for its production becoming more and more favorable as we go westward to the Mississippi valley, and there, in its greatest mass and the highest development of its fauna, we leave it passing beneath the more recent formations of Iowa with a thickness of six hundred feet.

In the succeeding order, the Onondaga Salt group follows, parallel in its outcrop and so far as we know coëxtensive with the Niagara group through New York, Canada West and thence to the Mississippi valley; and again southwesterly parallel with the Appalachian chain. Although having this great geographical extent, it has generally a moderate thickness and in some places has not received the recognition of a distinct group. Generally the deposit is that of a shallow sea, while in New York and Canada West it presents the features of a deep basin deposit. While thinning out on the eastern limits of New York to a thickness of thirty feet or even less, and westerly to less than one hundred feet, it still has in central New York a thickness of 1000 or 1200 feet; showing that there was a deep depression, caused in all probability by the lateral

pressure on the east, which elevated the rim of the basin on that side rather abruptly while it sloped more gradually to the west. This lateral pressure on the east is apparently caused by the gradual contraction which produced the more abrupt foldings of the crust in the region of New England and eastern New York, thus showing most conclusively that changes of level and modifications of the form of the ocean were going on throughout these periods and that librations extended much beyond the actual evidences of their action at this time.

I have chosen to speak of these formations where we know them fully and have trusted to no doubtful observations or evidence not fully sustained by facts. You will have perceived that thus far the Silurian sea has spread over all the known area of the older rocks on this side of the Rocky mountains. In the period of the Potsdam sandstone and lower limestones the depth was very uniform throughout, as would appear from the fauna. In the period of the Hudson River group we trace the deposits, as we believe, to greater depths than animal life of a molluscan type can flourish, while further to the east were shallower seas. In the Niagara period and the two succeeding ones, the ocean presents a pretty uniform character over a westerly extension of one thousand miles from one meridian, while we have evidence of libration on the east and particularly in that portion occupied by the sedimentary portions of the Hudson River group.

The lines or exposed areas of deposit mark the ancient ocean limits. When the conditions were favorable for sustaining life we look to the fossils for the indications of the changes of area and depth of this ancient ocean. In the absence of life, we seek an explanation of these changes of area and depth, in the direction and force of currents as indicated by the nature and extent of the sedimentary deposits.

If we now look to the next succeeding group of strata, which we term the Lower Helderberg group, we find the direction and extent of its area almost at right angles with those immediately preceding it, and parallel to that great line of sedimentary deposits marking the crest of the Hudson River group. From the investigations of the Canada Geological Survey, the Lower Helderberg group is known on the northeast at the extremity of Gaspé, resting unconformably upon the rocks below; thence to the south-

west more or less interruptedly it pursues its course, here at our feet in the bed of the St. Lawrence appearing in outliers which rest unconformably upon the upturned edges of the lower beds of the Hudson River group. Again in the Hudson valley above the Highlands lies the same series resting upon the shales and shaly sandstones of the Hudson River group, while the continuation of that group to the northward in its high crest of the Shawangunk is cut off by the erosion of the Hudson valley. Tracing the group but a little way westward, it passes beneath the Upper Helderberg in apparent conformity to the nearly horizontal Hudson River rocks below and those which succeed it. Southward we trace this group of strata through Pennsylvania, Maryland and Virginia to Tennessee, where its horizon is well marked. But notwithstanding this immense extent, from Gaspé to Tennessee it rarely extends more than a hundred miles to the westward of the Hudson valley in New York though having a greater width to the southwest. It therefore presents a belt of fossiliferous beds of about fifteen hundred miles in length from northeast to southwest. Here then we have the entire area of the west and northwest, as far as we know (with the exception of a very small area in Illinois and Missouri), entirely destitute of this formation or of any representative of it in the series. Was this vast area a deep sea or dry land? If a deep sea, we should expect to find the fine sediments of the shoreward formations drifted out into it to some extent; but these we nowhere find. In a few instances we have seen the lower beds apparently water-worn and broadly furrowed as if exposed to shore currents or even to littoral action, but of these conditions we have too little knowledge to speculate. But it still appears to me that we must admit the conclusion that at this period a large proportion of the area on the northwest of the line I have indicated must have been above water.

Thus while the trend of the limestones of the Niagara and the two succeeding calcareous groups has a general east and west direction, this limestone of the Lower Helderberg has a northeast and southwest trend. While the Lower Helderberg limestones by their fossil relations are Upper Silurian and intimately related to those below, the trend of greatest accumulation of the two is nearly at right angles.

The line of deposit of the Oriskany sandstone and Caudagalli

grit was coincident with that of the preceding limestone deposit and overlies it. The sandstone, indeed, stretched somewhat farther to the west and lay beyond the edges of the limestone upon the upper members of the Onondaga salt group; still the same area is occupied, and the line that admitted of the formation of coral-reefs in the Lower Helderberg period has become again, as in the period of the Hudson river group, the line of currents and of sedimentary deposits. This sandstone formation which in Gaspé is more than one thousand feet thick, and in Pennsylvania according to Professor Rogers, seven hundred feet thick, soon thins out to the westward. In the Helderberg mountains it has a thickness of not more than thirty feet, and often less than three feet, while farther west in central New York it occurs in a few isolated patches of a few feet in extent and less thickness, filling some small depressions in the upper beds of the Onondaga salt group. Here we have apparently again returned to the conditions of northeast and southwest accumulation of sediments deposited in the line of the Appalachian chain.

The Oriskany sandstone is the period in which acknowledged types of Devonian fossils are introduced. It lies parallel to the preceding Upper Silurian limestones and at right angles to the great mass of the succeeding fossiliferous Devonian beds. These types which were introduced in the period of a sedimentary rock which was deposited along a line from northeast to southwest, and cut off from the succeeding group by a non-fossiliferous belt (the Caudagalli grit), nevertheless appear and flourish in the next formation whose trend is nearly at right angles to the preceding, and the vital manifestations go on irrespective of the change in the physical conditions.

Succeeding the period which I have just described, where the sediments have a northeast and southwest direction, we have to contemplate a calcareous formation whose known trend along the line of outcrop is nearly east and west, stretching from the Hudson valley through New York and Canada West, thence across Michigan, Indiana and Illinois, to the Mississippi river and into Iowa, having an outcrop parallel and apparently conformable in position to the rocks of the Niagara, Galt [Guelph] and Onondaga salt group. The belt of formations just noticed as the Lower Helderberg limestone, the Oriskany sandstone and Caudagalli grit is

an important one along the eastern outcrop, but in the west these formations are entirely absent, and what is more remarkable, their absence is not noticeable, and for all we can determine on the ground, the sequence is as perfect in the west as at any point in our palæozoic area.

The calcareous beds before alluded to, including the Schoharie grit, constitute the Upper Helderberg group. The Schoharie grit has very little western extension, but is known for at least two hundred miles in a southwest direction. The limestones of this group mark a line of terrace from the Hudson river westward throughout New York, and present one of the best marked and most extensive physical features of the entire state. It is true the attenuate easterly margin of this group may be traced from the Hudson valley to Tennessee, and the same is true of the Niagara and Onondaga salt groups, though the line of greatest force is from east to west, and the whole series presents but a meagre thickness in Tennessee.

Pursuing our investigations southward from this limestone terrace, we find in eastern New York and in Pennsylvania, first: a series of shales and shaly sandstones; then we have a series of sandstones and shaly beds; and above these, shales and sandstones all of which may with little difficulty be included in one group as has been done in the geological survey of the latter state. But in central and western New York we find the lower part of the series becoming more shaly, and finally more calcareous, so that in the western half of the state it is a group of calcareous shales with beds of limestone known as the Hamilton group, and containing a set of fossils peculiar to itself.

The central portion of the mass becomes better defined as a series of shales and flagstones, with some heavy beds of sandstone which are well marked, though nearly destitute of fossils. This group of strata has received the name of the Portage group.

At the same time the third member has become defined as a set of shales and shaly sandstones with a peculiar assemblage of fossils, and has received the name of Chemung group.

When we follow these successive groups of strata to the westward we find them gradually thinning out. The Hamilton group traced through Canada West, Michigan and Illinois to the Mississippi, there presents a thickness of some fifty or sixty feet, while

in eastern New York it is scarcely less than fifteen hundred feet, and is a thousand feet thick in the central part of the state. The Portage group, which in its defined limits in central and western New York has a thickness of one thousand feet or more, is scarcely represented in the Mississippi valley.

The Chemung group, to which in New York I would not attribute a thickness of less than twelve or fifteen hundred feet, is often represented in the Mississippi valley by some thirty to fifty feet, and sometimes by one or two hundred feet of shaly sandstone and beds of limestone. These formations together in Pennsylvania have a thickness, according to Professor Rogers, of five thousand feet to the southwest.

Now regarding the formations in this aspect only, we should conclude that here were transverse currents carrying materials from the east to the west, and this conclusion was reached before we knew anything of the structure of the New England mountain series, and before we understood as we now do the geology of the northeast. The researches of Sir William E. Logan on the peninsula of Gaspé have shown that the *equivalents*, and in fact the *continuation* of these groups, as a part of the Devonian system, have an enormous development in an unaltered state, and in the condition of coarse sediments of at least seven thousand feet in thickness and with a southwesterly trend.

If we follow the line of trend we are taken along the direction of the White mountains, thence into Massachusetts and New York, where we are taught that higher metamorphic sediments overlie authentic Lower Silurian, until we find that the unaltered and undisturbed Hamilton, Portage and Chemung groups of New York are but the prolongation of the Gaspé sediments which have become folded and metamorphosed in the region of the White mountains, but which reappear on the west in their unaltered condition and highly fossiliferous character as soon as out of reach of this disorganizing action of metamorphism.

Instead, therefore, of finding here any *new* force or diverse transporting currents, we have simply a return of the great transporting agencies of the Hudson river period, from the same source apparently in the northeast, and prolonged in the same direction to the southwest, parallel and coëxtensive with the Appalachian chain. The finer materials were spread laterally

through the more quiet ocean, till they nearly disappear by thinning out in the Mississippi valley. You will see, gentlemen, with what an enormous amount of sedimentary deposits I am loading the eastern side of the future incipient continent, an amount too great perhaps for credibility, but I have not yet done. From the same source and in the same direction has come the vast amount of sediment forming the Catskill mountain group, with the capping conglomerate, having a thickness of more than three thousand feet in New York and six thousand in Pennsylvania, while the group has entirely thinned out before reaching the western limits of the state, and no vestige is known of it in the Mississippi valley.

Beyond this are, according to Professor Rogers, three thousand feet of red shale at the base of the coal measures and extending parallel to the Appalachian chain. The origin of this shale has not been satisfactorily traced in the northeast, but there is no difficulty in finding a source for this and other sedimentary deposits of the Coal Measures, and an evidence of the same transporting force in the fourteen thousand feet of Lower Coal Measures found by Sir William E. Logan to exist in New Brunswick.

Now, gentlemen, I am satisfied I have wearied your patience too much in these details, but I could not present a simple conclusion without first giving you the data in full.

I have shown you incidentally, as we progressed, that the Hudson river group became disturbed, folded and upturned before the deposition of the Niagara, and still more before the deposition of the Lower Helderberg group which has its trend parallel to this great belt of formation, and which lies upon the upturned edges of the Hudson river group, from Gaspé to the Highlands of the Hudson.

Indeed, I believe the origin of the coarse materials of the Medina and Clinton groups may be due to disturbances among the preëxisting beds of the Hudson river group, and probably derived from the breaking up of the conglomerates of this group in its northern prolongation. Sir William E. Logan has likewise proved that with the commencement of the foldings and plications of these lower strata commenced the metamorphism of the same. And here I cannot forbear from alluding for a moment to the views of Mr. Hunt

upon metamorphism. Laying aside all assumption of causes not known to exist, and ignoring the supposed effects of that imaginary nucleus, he has proceeded, from known and demonstrated facts derived from existing conditions, to explain most satisfactorily certain processes in metamorphism; and, instead of appealing to an unknown source for the ingredients of certain metamorphic strata, he has demonstrated the existence of the same elements in the unaltered beds which are known to be of the same age and prolongations of the same strata.

The system of folding and plication therefore, which has been attributed to later violent action, has been going on from the commencement of the Upper Silurian, and though its later phases include the Coal Measures, it has nevertheless been produced by continued action from a long anterior period. Nor has it ceased with the coal, but I believe has been continued in a less degree since the deposition of the sandstone of the Connecticut valley. But even for one moment admitting the supposition that these foldings and contortions have been for the greater part produced at a single period, let us inquire whether this has had anything to do with the production of the mountain chain, or whether this system of mountain-making by upheaval, overturning, etc., is a system of mountain-making, and whether rhetorical eloquence has not made us believe the language without testing the facts. I must assert that it has nothing at all to do with it—that so far at least as regards our mountain chains, the principle in its application is false.

You have seen how, age after age, by the simplest of all processes, the immense sedimentary deposits have been transported and distributed along the line of our great eastern mountain chains; and with the accumulation of sediments this line was depressed time after time, as new deposits were piled upon it; and now at the end of the Devonian period, or at the end of the Coal period, has the whole continent been raised to its present elevation. If there were no folding or contortion, and if the strata of the Alleghany mountains were as undisturbed as those of the great plateau of the west, what would have been the result? Simply that, instead of the Appalachian mountains of six or seven thousand feet in height, you would have had a simple stratified pile of more than twenty thousand feet from the base of the Potsdam to the summit of the Devonian.

But have folding and partial upheaval along mountain chains nothing to do with the grand elevation? I reply, nothing at all; but, on the contrary, every one of these disturbances lessens the effect and degrades the elevation. It is *original deposition* that not only gives direction to our mountain chains, but amount of deposition that determines their elevation; or, if you will reduce it to the simplest terms, we may state that *elevation is due to deposition*, and there can be no great degree of elevation without a corresponding amount of previous deposition of sediments.

Now I conceive this to be the simplest system of mountain-making that can be proposed, and it can be shown to be applicable not only to the Appalachian chain, but to other mountain ranges of our continent.

Let us see, moreover, whether the same simple principle, somewhat extended, may not be applied to continent-making. We admit that the Appalachian chain has given mainly the eastern outline to our continent, and that original deposition has given origin to this mountain chain. The currents of the palæozoic ocean have determined the directions of these sediments, and consequently of the mountain ranges and the outlines of the continents.

A series of strata similarly deposited by an ocean current, but nearly at right angles, mark the western outline of our continent; and though modified by subsequent depositions, they are nevertheless fundamentally of like origin, due to the same causes, giving origin to a mountain chain and the outline of a continent.

Now, if we extend our observation to the Mississippi valley, a thousand miles to the west, where we have essentially the same set of strata as those forming the Appalachians, we find no mountain ranges. I shall be told that it is because there are no upheavals, but this is not known to be true. Several axes and extensive faults cross the valley of the Mississippi nearly at right angles to the Appalachian chain, elevating all the strata from Lower Silurian to Devonian. Nevertheless, though the strata are elevated from ten to thirty degrees, they produce scarcely a perceptible feature in the prairie country.

Uplifting has not produced elevation, but has rendered the strata liable to degradation, by breaking or weakening them along the line of fracture or folding. I have already shown that the groups of sedimentary strata composing the Appalachian chain have thinned

out to a few hundred feet at most on the Mississippi and there are consequently *no materials* of which to make mountains in the great Mississippi plateau. The general height of the country gives the entire elevation produced by the thickness of the beds, and there can be no more. We could have no mountain chains along a great plateau like this, however much the strata may be folded and contorted, unless there should take place a dislocation of the older Laurentian or Huronian rocks from below; and although such a phenomenon would be in accordance with geologic *theories*, respecting the upheaval of mountain chains, yet I suspect that careful search would prove that no such upheaved chain of mountains exists upon the surface of the globe. In fact, that the upheaval of mountain chains, except in the sense that the elevation of a continent is upheaved, has never taken place. In this connection let me offer an example in point in support of what I have said. It has been stated that the White mountains are composed of accumulations of sedimentary deposits of Silurian and Devonian age, having altogether in their undisturbed condition, a thickness of from fifteen to thirty or forty thousand feet (or more, if I follow authority), while at the same time the White mountain chain little exceeds six thousand and the Green mountains not more than four thousand feet in height. Now in tracing this mountain chain to the southward, we observe, on the west side of the Hudson river above the Highlands (which are produced by an area of older rocks), the Catskill mountains reaching an elevation of about four thousand feet, and stretching away to the westward in gradually declining isolated hills. These have been regarded as part of the Appalachian chain but they have nothing to do with it so far as being involved in the disturbance of the strata. They stand alone as an isolated group, of which the nearly horizontal pile of strata gives this great elevation. The disturbed Hudson River group makes one or two hundred feet of their base. *Deposition* has here given *elevation* without loss or degradation by any of those foldings and plications which are supposed to lie at the foundation of mountain-making. Had the strata of the Green mountains and White mountains remained undisturbed, and in the original condition of deposition, they would have overtopped the Catskills by several thousand feet.

Original deposition, then, directed by ancient currents, has un-

questionably determined the direction and elevation of our mountain chains.

The time on this occasion will not allow me to extend the application of the same principle in unravelling and explaining complexities, that may often occur — when, during the depression of a continent or of a portion of it which may afterwards form a mountain chain, transverse or diagonal currents may cause such an amount of deposition as to produce a great complexity of structure, when the continent shall become elevated.

The simplicity of the North American continent is due to the originating causes producing the direction of the palæozoic currents; while a large part of the eastern side has not been subsequently submerged, to any great degree, and has thus escaped the causes of complexity. The wide extent of later deposits in the interior of our continent, which give contour to that portion, was in a great degree paralleled with the eastern deposits. The forces which we say have rent the eastern, the central and the western, are merely the vast accumulations of material which become elevated, perhaps simply from the great accumulation, thus rendering the escape of heat from the interior slow or impossible. And in this statement, it must not be forgotten, that the central portion of our continent, and some parts of the western portion, were not continental, but submerged areas, till after the Cretaceous period, and to some extent also, as late as the Tertiary period; and if these have greater elevation it is due to greater deposition.

Although it is not necessary to introduce the Coal Measure deposits into the Appalachian system for the purposes of explaining their elevation, they are nevertheless complicated with it, in a great degree. But all the phenomena, so far as regards folding and plication, and the production of the northern mountains as well as a large part of the Appalachian system, do not include the Coal Measures in the elevation. On this account I would call your attention to some facts connected with the deposition of the coal formation in the United States. But before proceeding to do so, I must dwell for a moment on the Carboniferous limestones, or those great calcareous formations which, in the valley of the Mississippi and the southwest, lie beneath the Coal Measures.

In tracing the origin of our sedimentary formations from the northeast, we do not find any cessation from the beginning of that

period called Devonian till we come to the Coal Measures. The whole country in this direction would seem to have been submerged, and the course of these ancient currents to have continued with little interruption.

In the Hamilton, Portage and Chemung groups, we first find *land plants* and these remains increase towards the northeast, while they are rarely or never known in these formations to the west of the limits of New York. In Canada, however, similar plants are known, and the account of a specimen in the collections of the Survey, published by Mr. Dawson, shows that dry land may have been near, and we have every reason to believe that dry land capable of producing vegetation did first appear in that direction and in the early Devonian period. While in the east, the Catskill mountain rocks, and the red shale beneath the anthracite Coal Measures, were being deposited, we have in the west no representatives of those rocks; the Carboniferous limestones beginning their deposition directly upon the Chemung group.

The condition of the ocean in the west during these periods is worthy of attention. Upon the cessation of the sediments of the Chemung group, commences a limestone formation of great extent, and marked by numerous species of fossils. By a well-defined line, this lower limestone is separated from a succeeding one, marked by a wholly distinct set of fossils; while its greatest development lies to the south of the area of greatest development of the first. In the same manner a third, less strongly defined; and with an intermediate sandstone formation, still another limestone succeeds, and this with its northern limit far below or to the south of the first and second, and its greatest development is to the southward of the preceding members.

Now the simple explanation of this seems to be that the sea, in which limestone deposits could be made, was contracting on the north and gradually extending to the south and southwest, while we have evidence to prove that much dry land existed to the north,—evidence which I shall presently attempt to show. This fact of the gradual expansion of the ocean to the southwest, and its contraction on the north, is significant when taken in connection with the succeeding formation of the Coal Measures; for which reason I introduce here the notice of these facts.

It might appear unnecessary while so much has been written,

and so well written, upon the subject of the Coal Measures of the United States, to offer anything in this place, but I believe I can present one or two points of interest in this connection.

I have already called your attention to the fact, that the sedimentary materials of the coal formation have their greatest expansion to the northeast, and that we find the earliest record of land plants in that direction. The Coal Measures, as a whole, though widely expanded, show a gradual thinning-out to the west and southwest, and in Iowa and Missouri have become greatly attenuated as compared with the same formations in Pennsylvania, Ohio and Kentucky.

I have just now reverted to the fact that on the north and west, we have evidence of dry land, or of land raised above the sea previous to the deposition of the coal. While in Pennsylvania, Ohio, etc., the Coal Measures apparently rest conformably upon the strata below them, in Iowa and Missouri they rest unconformably upon the rocks below. While the prevailing dip of the older rocks is to the southward, there are nevertheless numerous faults and axes which uplift the strata in a direction from N. W. to S. E. or at right angles to the Appalachian chain.

In the west these ancient rocks of Lower and Upper Silurian and of Devonian age have, previous to the deposition of the coal, presented numerous inequalities of surface, partially due to the broken strata produced by elevations and faults, and partially by subsequent denuding action.

In Illinois, Iowa and Missouri we find the Coal Measures spread horizontally over the uplifted edges of these older strata, sometimes barely filling the depressions and inequalities of original surface, giving origin to numerous little circumscribed basins, often of only a few yards in extent. And not only do we have the evidence of ancient worn and unequal surfaces of this ancient land, but we find the sand mud, and carbonaceous matter often filling fissures, and penetrating into ancient caverns, which must have been worn while these older strata were near to or above the level of the sea. The wearing down of the crests of low axes, and the broken margins of faults, prove the existence of a powerful eroding action, and as we know that at this period the sea was gradually retreating to the southward and the land rising on the north, the motion of the abraded materials would be in the same direction.

There are other phenomena connected with the coal period, which are of much interest. In Pennsylvania and Ohio, we observe several intercalated bands of calcareous shale or argillaceous limestone, charged with marine fossils, thus indicating that the area of the Coal Measures was sometimes depressed beneath the sea. These bands of limestone with marine fossils are known in the Coal Measures of Ohio, Illinois, Iowa and Missouri. The limestone of this period in Missouri has been usually referred to the Carboniferous limestone, not recognizing these beds as distinct from those below the coal; and it was not until the publication of the Missouri Report that Professor Swallow clearly indicated the position of the beds on the Missouri River as a part of the true Coal Measures.

In the meantime and for many successive years, fossils of Carboniferous age had been found in the far west along the Rocky Mountain range from as far north as Fort Laramie and the Great Salt lake, by Capt. Stansbury, and at numerous intermediate points by the Pacific Railroad Surveys and other United States explorations, and on the Southern Boundary line and in New Mexico by the Boundary Commission. Dr. Roemer also had discovered some fossils from the Carboniferous limestone in Texas. We had therefore a knowledge of the vast extent of a limestone along the Rocky Mountain range from latitude 43° north, to about 30° south, containing fossils referred to Carboniferous age, and the rock was almost by common consent referred to the age of the true carboniferous limestone and has been cited as Lower Carboniferous limestone.

Having had occasion during the two preceding years to give some attention to the rocks of Carboniferous age, and more particularly during the past year to a comparison of the fossils, I was surprised to find that some of these southwestern species were identical with those from the well-developed carboniferous limestone in the Mississippi valley, Iowa, Illinois and Missouri to Alabama. Furthermore a careful comparison of species collected by myself many years since from the limestones of the Ohio coal measures, with others collected by Mr. Nicollet, Capt. Stansbury and by Messrs. Meek and Hayden from the limestones of the Missouri river, showed a most positive identity of species. A farther comparison of all these with the carboniferous fossils from

some of the Pacific Railroad Surveys and those of the Boundary Survey proved such positive identity of species as left no doubt that all the limestones of the southwest, that is, in the approaches to the Rocky Mountains and beyond them, are of the age of the coal measures. A later comparison of the collections of Marietta College has enabled me to demonstrate the identity of several of the species described by Dr. Hildreth with those of the southwest, and it may be unnecessary to add that these are Coal Measure fossils.

These limestones, then, of the southwest, spreading over an area many times as large as all the Coal Measures of the remainder of the United States, are the greater development of those thin bands of limestone with marine fossils that mark certain lines in the Coal Measures of Ohio, etc. These bands of limestone, from insignificant beds of a few feet in thickness, have extended to several thousand feet in thickness and mark great lines of mountain ranges, and, lying often nearly horizontal or dipping at various low angles, present vast cliffs along mountain passes towering thousands of feet above the plain below. And these, gentlemen, are limestones of the Coal Measures, while the productive Coal Measures are unknown over this vast region. With what propriety, then, can we say that the coal period is preëminently the period of vegetation, as is done to sustain a theory that it marks one day in the creation of the world, or to sustain another view that coal vegetation gives the character to the entire palæozoic series?

I regret, gentlemen, that I cannot bring you some very brilliant theoretical result from all these data; but the very simple explanation of all is, as I have shown you step by step, that the northeasterly portion of our continent was either already dry land, or was just rising from beneath the water at this period; that as low ground just emerging from the ocean, and at intervals deluged with sand and mud and lying little above the water level, it sustained a luxuriant growth of vegetation; while the gradually retreating ocean still spread over by far the largest proportion of the continent on the west and southwest. The accumulated reefs of limestone, three thousand feet in thickness, attest the long period of quiet, and freedom from irruptions of sedimentary matter upon these regions. At intervals the unstable position of the newly made dry land permitted the incursions of this southwestern ocean, in a shallow ex-

pansion over the great Coal Measure area, only to return again, upon the gradual elevation of the surface, and thus once more to become a low land fitted for the production of plants.

This subject followed out promises us some of the most interesting and beautiful results, and will show us how the librations of the surface have been connected with the destruction and reproduction of the successive floras of the coal period.

There are numerous interesting generalizations arising legitimately from the facts I have brought before you, but of which time will not permit a notice. In no direction, however, more intimately than in that of palæontology, do these facts and physical deductions point to interesting and important conclusions. There is one fact, which in this connection I cannot omit, in regard to the fossils in these intercalated beds of limestone in the Coal Measures, and this is—that they are species of the same genera and many of them of allied forms to those characteristic of the limestones below the Coal Measures, or of the Devonian and Silurian limestones; showing conclusively that the condition of the ocean did not essentially vary through this long period, and the only difference between the coal period and that of the preceding was that there was a larger area of dry land giving origin to the great growth of vegetation of that period. Moreover, the large area of this ocean formation, characterized by a marine fauna of the same period, compared with the relatively small extent of the areas of carbonaceous matter and terrestrial vegetation, shows that so far from being essential, they are rather incidental conditions of the Carboniferous period.

CONCLUSION.

In the progress of the earth from the beginning of animal life to the present time, we are but watching the progress of an ocean bed to its final development as a continent,—and for the most part, every step has been one of progress towards the present condition. Whenever any portion of this ocean bed has been raised above the surrounding sea, land plants have taken possession of that portion. If it has remained long enough above the surface, land animals have become its denizens. As a matter of course, the higher land animals did not first appear, for even now, simi-

larly situated lands do not sustain the higher organizations. The occurrence of mammalia in the Jurassic period is no more irrelevant to that period, than the occurrence of plants in the coal period. It is a fact perfectly in accordance with all the rest of our legitimate information, that the organic conditions have always been coincident with the physical conditions.

Geology, if we would let alone grand theorizing, is a simple and beautiful study, in which we see everything evolved naturally and harmoniously, without at any time great and sudden changes. We remark those changes as one who having viewed a city in its progress, should fall asleep for a century and afterwards behold the difference. But to one who could have seen stone laid upon stone, and each edifice completed singly, it would have had but the aspect of natural and quiet progress.

NOTES.

PAGE 37. The gneiss cited as Laurentian and containing bowlders of preëxisting gneiss I have seen in the region of Lake Superior, and they were referred to the Laurentian age on the authority of Sir W. E. Logan. I am informed by Dr. T. Sterry Hunt that these masses are of Huronian age and carry boulders of stratified Laurentian gneiss. Although this shows that we have carried back the stratified gneiss only to the Laurentian period, it proves nothing in favor of a granitic nucleus nor of extruded granites of igneous origin.

PAGE 41. A comparatively quiet condition, during a large portion of the period of its accumulation, is indicated by the uniform fine-grained character of the quartzose sand, which extends over large areas. The tracks of Crustaceans, found by Sir William E. Logan upon certain surfaces of this rock in Canada, appear to have been at about the same level along the strike of the formation for 400 miles. The deposit was evidently made in shallow water, and from the mark of a wind-ripple found upon the surface in connection with these tracks at one locality, it is evident that the sand had been laid bare at the ebb of tide.

The formation in Canada has a thickness of from 70 to 600 feet, and an extent of a thousand miles flanking the Laurentides from Bellisle to its extreme southwestern extension in the Province. "The hills, at no very great distance from the outcrop of the Potsdam formation, rise to heights varying from 500 to 4000 feet; and while the sand at their base lay between the ebb and flood tide, the flank of the Laurentian mountains must have formed the coast of the Lower Silurian sea. As has already been stated, these hills extend from Labrador to the Arctic Ocean, and we can thus trace out the ancient limit of the ocean for 3500 miles."

This formation extends southward through New York, Pennsylvania, Virginia, and notably through Tennessee, and occurs flanking the older metamorphic rocks in Texas. The formation is extensively developed in the States bordering the upper Mississippi and in the upper Lake Region. It likewise constitutes an important feature in the Geology of the Black Hills country.

The character and thickness of the formation in New York, along the

eastern flank of the Adirondacks, is similar to that existing in Canada. In the Mohawk Valley at the southern extremity of the Laurentian gneiss it is often scarcely recognizable, and never attains more than a few feet in thickness in any of the exposures. In Pennsylvania the maximum thickness of the formation is from 2500 to 3000 feet, while in some localities its thickness does not exceed twenty-five feet: — this variation being due, in some degree at least, to the original inequalities of the sea bottom.

In Tennessee, according to Professor Safford, the Potsdam group has a thickness of more than 20,000 feet; but this includes the conglomerates below and the magnesian limestones above.

The source of the Sandstone flanking the Adirondacks has been attributed to the subjacent Laurentian gneiss, and it has been suggested that long continued trituration has broken up and separated the mica, hornblende, etc., which have been floated away, leaving only the siliceous grains to form the great deposit. It may, however, be questioned whether the gneiss has been the chief source of material. In the great conglomerate or breccia (for the fragments are mostly angular), at the base of the Potsdam on the southeastern flank of the Adirondacks, the fragments of gneiss are few and small, and the greenish material, which may have been derived from the mica and hornblende, is in small proportion. By far the largest constituent of the mass is of quartzite, corresponding with a similar material found in many places lying upon the gneiss, and sometimes referred to the Potsdam, while in some localities it appears to have more intimate relations with the gneiss below.

Fragments of feldspar are sometimes found embedded in this conglomerate, and, in the more interior localities, a larger proportion of fragments is evidently derived from the underlying gneiss rock. In Tennessee, Professor Safford represents the lower portions as "coarse gray conglomerates, talcose, chlorite and clay slates, repeatedly interstratified, all having a semi-metamorphic aspect." But it should be recollected that the adjacent metamorphic rocks may be of newer age than the Laurentian. Again "the pebbles are quartz and feldspar mainly, generally rounded, but sometimes more or less angular." The conglomerate "usually contains more or less chloritic and talcose matter in its interstices."

Along lines of the most active erosion, no large deposits are made, and those which are made are of a coarse character; the great accumulation is just outside of the limits of the most active forces.

The underlying conglomerate or breccia is widely disseminated, having a greater thickness at the south (in Tennessee) than in the north. This may be accounted for by the gradual subsidence of the entire continent, or the eastern portion thereof. The deposition of sediments apparently began at the south, coincident with the subsidence, and there was a recurrence of similar conditions during the whole period of depression, so that at the base we have a conglomerate which is followed by a sandstone; and in Tennessee, where the subsidence was greatest and farthest removed from the final source of sediments, we have the greatest thickness, and the uppermost beds are shales, indicating deep water and distant source of material.

In the original nomenclature the term Potsdam sandstone included conglomerates and quartzites resting on the Laurentian gneiss, with the sandstones above these, to the base of the calciferous sandstone (magnesian limestone). It has been found, however, in some localities, that there is a gradual passage from the Potsdam to the calciferous; and above the latter a repetition of the previous conditions in the deposition of a fine grained sandstone in the northwest known as the St. Peter's sandstone; while in Missouri there are several alternations of sandstone and magnesian limestone, showing that similar conditions were recurrent to the close of the series.

It seems pretty clear, therefore, that while the magnesian limestone was being deposited in mid-ocean there may have been a deposition of sili ceous

sand along the coast line or littoral zone, giving a continuous series of sandstone layers, or, as sometimes occurs, with thin calcareous interlaminations. We can conceive it possible that the Potsdam fauna may have lingered along the shore lines, even after the close of the deposition of the magnesian limestone.

In Tennessee, Professor Safford includes all these in the Potsdam group, recognizing three divisions; the lower division of conglomerates and slates; the middle, the Chilhowe sandstone, with intercalated beds of dolomite; the upper including the Knox shale and the Knox dolomite which closes the series. The whole constitutes a series of deposits most naturally grouped together, as belonging to a single geological period, and they are represented as conformable throughout, and are equally involved in the later movements which have folded and plicated the mass as shown in the mountain exposures. The series, wherever of considerable thickness and exposure, is susceptible of subdivision, and Dr. Owen suggested a triple division of the mass below the magnesian limestone in the upper Mississippi Valley, while a different subdivision is applicable to Missouri. Thus far no one has successfully attempted the correlation of the subdivisions recognized in different parts of the country.

The relations of the fossiliferous beds at Troy, N.Y., referred to the lower Potsdam, have never been satisfactorily determined. It is a matter of mere conjecture. The fossils are from calcareous masses embedded in slates whose age and relations are unknown and which in fact cannot now be separated from those of the adjacent Hudson River group. Therefore it is unsafe to attempt to correlate, with these beds, strata in other portions of the country.

(3) PAGE 42. The northeastern source of the mechanical sediments has been questioned by several writers. The greater thickness of many of the formations in Pennsylvania, than to the northward, has induced Professor Lesley to infer that the source of sediments has been in the southeast, instead of the northeast as I have suggested. By northeast I would be understood to mean northeast of the centre of the Appalachian chain. The rapid attenuation of all the sedimentary deposits above the Potsdam, to the south of Maryland, and the increasing proportions of the calcareous formations in the same direction, seem to me to indicate that the great source of sedimentary deposits was to the north and northeast. In the southwestern continuation of the Hudson River group we find it passing gradually into calcareous material. Taking the entire series from the base of the Hamilton to the top of the Catskill, including the upper member or Pocono sandstone, embracing altogether a thickness of more than 10,000 feet, we find the whole represented in Tennessee by a few hundred feet of strata. At the north the strata of the Hudson River, Medina, Clinton, Hamilton, Portage and Chemung groups are largely arenaceous; while in the south the corresponding formations except the upper ones are mostly calcareous. Had the source of the sediments been from the south, we should have to assume that the coarse deposits were made at a great distance from this source and that they were transported across a large area which now preserves only the evidence of gentle forces and open sea accumulation. It would seem, therefore, that this proposition is neither necessary nor satisfactory, and that the source of sediments was probably from the north or northeast.

PAGES 44, 45. In 1857 the Canadian Survey had recognized the entire belt of slates, sandstones and conglomerates on the southeast side of the St. Lawrence, including also a considerable proportion of the metamorphic rocks farther to the eastward, as of the Hudson River age, and the subject was thus treated in the address.* Subsequent investigations proved a large proportion of this area to be of older formation, but notwithstanding this

* Prof. Rogers, and other geologists, had before this time recognized the metamorphic rocks of New England as of palæozoic age.

fact, the reasoning regarding Anticosti and its relations still remains correct. The island is a mass of strata in which calcareous matter prevails to the exclusion of coarser sediments.

Owing to the more northerly trend of the Laurentides from the Pt. de Monts, a wide space to the northeast has been occupied by the ancient palæozoic ocean in which were deposited the several members of the series from the calciferous to the Niagara inclusive, in quiet succession.

The intermediate space between Anticosti and the Mingan Islands on the north has been occupied by the upper part of the Birdseye and Black River limestones, and by the Trenton limestone proper, the Utica slate and lower part of the Hudson River group; the upper members of the latter, to nearly 1000 feet in thickness, forming the northern shore of Anticosti. This gives to the lower formations, from their base to the top of the Hudson River group, a thickness of more than 2600 feet. A large part of the island, however, is composed of the succeeding formations. The Anticosti group consists of representatives of the Medina, Clinton and Niagara groups, but these are everywhere calcareous, having a maximum thickness of nearly 1400 feet.

The sequence of open sea deposits of this region, from the Trenton to the Niagara groups, has, therefore, apparently been more complete and less interrupted than at any other point within our knowledge. Unfortunately, the hiatus between the Mingan Islands and Anticosti leaves us without positive knowledge of the nature of the deposits in this interval. The coarser sediments of the Hudson River group, in its typical localities, occur in the upper part of the series, and since here we have the higher portion of the group represented by nearly 1000 feet of calcareous beds, it is fair to infer that it may have been calcareous throughout. In its physical aspect as well as in its fossil contents, the Hudson River group at Anticosti is comparable with the series of the same age in Ohio and Indiana, the northern part of Kentucky and in Tennessee.

Crossing from Anticosti to the peninsula on the south side of the St. Lawrence, we find the Hudson River group represented in its finer and coarser sedimentary deposits as known in its typical localities, and here as well as at Montmorenci near Quebec and elsewhere, complicated with the older slates, sandstones, etc., of the Quebec group. From Gaspé, therefore, by the south side of the St. Lawrence to Quebec, and thence to Montreal and by Lake Champlain and the Valley of the Hudson to the Highlands of New York and through New Jersey, and Pennsylvania to Virginia, we have this belt of sedimentary strata, usually more or less disturbed and complicated, and sometimes partially metamorphosed, but always well-marked and clearly distinguishable over an extent of outcrop of nearly 1500 miles.

PAGE 46, ¶¶ 3, 4. Of late the question has been raised regarding the unconformability of the Niagara group and subsequent formations, to the rocks of the Hudson River group below, and consequently the date of the foldings and plications of the latter have been controverted by geological writers. The original idea of unconformability was maintained by Rogers and Mather of the N. Y. Geological Survey while Emmons in one case explains the apparent want of conformity as due to faulting, while he gives another section showing conformability. Sir W. E. Logan everywhere maintained the nonconformability of these formations. I have held this view from my own investigations, though expressing some doubt in a single instance, but I have no longer any hesitation in my own mind regarding the matter, for wherever there may be *apparent conformability* it can only be delusive.

At St. Helen's island near Montreal, and elsewhere, the limestones of the Lower Helderberg rest unconformably upon the inclined and eroded edges of the Utica slate or other lower members of the Hudson River group, with an intervening conglomerate in which are found pebbles of Trenton limestone, Utica and Lorraine shales. The entire superficies

of the valley had been eroded previous to the deposition of this Lower Helderberg limestone. Numerous localities along the Hudson River valley present evidences of want of conformity between beds of the Hudson River group and those of the succeeding formations. Those which have been chiefly cited are Rondout and Catskill or between these two places on the west side of the Hudson, and Becraft's mountain on the east side. The more conspicuous sections are near Rondout, but one of the most interesting occurs at Austin's Mill near Catskill. At this locality the general aspect of the section presents an appearance of conformability between the formations below and those above. A critical examination, of the line of contact, however, presents not only distinct discordance of the beds, but in one place the line of junction is marked by a thin stratum of grit with rounded pebbles separating the sandstone of the Hudson River group from some calcareous layers of the age of the Niagara group. This point is in the trough of a synclinal and the lines of stratification are essentially parallel. At another place in the same exposure near the edge of the synclinal are presented the upturned edges of the Hudson River sandstones and shales, succeeded by a thin band of grit and breccia; and upon this rests a soft magnesian limestone of the age of the Onondaga Salt group, succeeded by the haydraulic limestone and members of the Lower Helderberg series.

The apparent conformity in many cases is an illusion; for, although the successive beds may appear to be in accordance, so far as bedding is concerned, a more careful examination may show that the strike of the one crosses that of the other, as is seen in the exposure at Austin's Mill, where the divergence of strike between the Hudson River strata and superimposed formations is at least thirty degrees at one point on the edge of the synclinal.

It should be noted that in the greater number of junctions described, between the Lower Helderberg and Hudson River groups, the limestones of the former group are often found resting on the shales and slates which characterize the middle and lower members of the Hudson River series, while the upper arenaceous beds of the group are wanting. Nor would absolute contact and parallelism of beds in this case prove conformity of structure. It is quite impossible that complete conformity should exist between the beds of the Hudson River group and any portion of the Niagara or Onondaga group, in the absence of the entire series of the Medina sandstone marls and the shales of the Clinton group; and when it is proposed to bring the members of the Lower Helderberg into parallelism with the Hudson River group, the absence of several thousand feet from the base of Medina to the top of the Salt group shows the impossibility of such conformity. Unfortunately, the absolute junction of the two formations has rarely been carefully sought — where a few inches of conglomerate or breccia may represent the interval between the close of the Hudson River and the beginning of the Helderberg period — an interval of thousands of feet in thickness and ages in time.

The beginning of the disturbance was at the close of the Hudson River period and the great thickness and extent of the red marls of the Medina indicate that the deposit was not made in an open sea. A vast interval separates the Lower Helderberg from the Niagara. The whole Onondaga period with its marls and dolomites of one or two thousand feet in thickness is interposed. The open sea was shut off and a region with salt lakes spread over the formations of the preceding ocean area for a thousand miles to the westward (as now evidenced in its salt and gypsum beds), while thinning out on the east. Over this eastern outcrop and far eastward swept the Lower Helderberg Sea, which laid down the limestones in the valley of the St. Lawrence, on the flanks of the Beloeil mountains, east of Montreal and from Gaspé* by Maine, New Hampshire and Vermont, and from New York to Tennessee. The break, shown by all these phenomena,

* The limestone in Gaspé is 2000 feet thick.

ending in the unconformable spread of Lower Helderberg, alike over the filled up Onondaga basins and uncovered Niagara and Hudson River groups in eastern New York; over eroded Utica slate in eastern Canada, and over older crystalline rocks from New England to Gaspé, marks the first great geological revolution from Potsdam time.

PAGES 49–51: The term *trend* of the formations was used to indicate, as far as known, the line of greatest accumulation or the line of transport of the material. Since this knowledge is derived from the outcrops, and much intermediate ground remains unexplored beneath superincumbent formations, it has been urged that the inferences may not always be technically correct; but I do not conceive it probable that any erroneous conclusions of importance are likely to result from such statements of the fact, —at least the actual outcrops furnish more reliable information than any system of hypothesis.

The Oriskany sandstone, from Gaspé to Virginia in its trend or line of greatest deposition, is essentially parallel to the Lower Helderberg. In New York it extends, as a marked formation, farther west than the Lower Helderberg, and is also found in Canada West. Both the Oriskany sandstone and Lower Helderberg group are known in southwestern Illinois. The eastern extension of the Oriskany is overlaid by the Caudagalli and Schoharie grits. In central and western New York these formations are absent, and the Oriskany was exposed to eroding influences during the time of their deposition, and until the beginning of the deposition of the Corniferous limestone. Through the central and western part of New York the Oriskany sandstone is found filling scattered depressions of limited extent in the preceding formations. Sometimes the lower beds of the Upper Helderberg group contain rounded and angular fragments of this sandstone, showing conclusively that there was considerable erosion of the strata below, previous to the formation of the limestone.

The sandstone is unknown (except as small isolated patches or nodules, or as fragments embedded in the limestone above) between Seneca Lake, in central New York, and the Niagara River, and yet near Cayuga, in Ontario, after an interval of 200 miles, there are outliers of the rock yielding an abundance of the characteristic fossils of the formation. It is, therefore, fairly inferred that the absence of this rock in many places may be due to erosion, while the remaining evidences of its existence might readily be overlooked.

PAGE 55. The Address has been facetiously criticised as proposing a system of mountain making with the mountains left out. The Address was not intended to propose any system of mountain making, but to show that mountain ranges were coincident with lines of great sedimentary accumulation. That this accumulation of sediments with its subsidence and consequent folding and plication, and the subsequent elevation of the mass and erosion of the anticlinals, had shaped the mountains; that no mountain elevations could take place where the sediments composing the area were thin; and that the mountain elevations were never equal to the vertical thickness of the strata composing them. I intended to imply that mountain elevation was due to sedimentary accumulation and subsequent continental elevation —trusting to the intelligence of my hearers and readers to interpret my suggestions.

From various sources giving the thickness, I stated that the maximum of the palæozoic sediments, entering into the formation of the Appalachian chain, was 40,000 feet. Perhaps it would be more prudent, as a basis of argument, to accept a medium and place the thickness at 25,000 feet (though the aggregate is much greater), out of which have come mountains of 5000 feet in height. It may not be easy to account for the manner in which the enormous erosion has been accomplished, for this could not have taken place beneath the sea, and the most natural explanation is that the eastern part of the continent has at some subsequent time been greatly elevated to allow of such erosion. I did not pretend to offer any new

theory of elevation, nor to propound any principle as involved beyond what had been suggested by Babbage and Herschel. I did not propose to discuss the theory of the contraction of the globe from cooling, or of the crumpling of the earth's crust from the gradual cooling and shrinking of the interior mass, because such arguments are not always philosophical for want of a basis in facts, and are always unsatisfactory as giving a very inadequate solution of the problem. This question cannot be properly discussed in a note.

I am satisfied that a region where the ocean bed, during palæozoic time, subsided so as to permit a deposition, under water, of more than 25,000 feet of strata, is sufficiently unstable in character to come up again to an elevation required for the erosion of the anticlinal valleys. This great subsidence alone may indicate that the area was one of weakness and liable to elevation or depression according to the action of the forces.

During the long palæozoic time the area of subsidence was in the Appalachian region, though clearly enough, during some portion of that time great uplifting occurred on the northeast, to be succeeded by subsidence which may have been equal to the elevation. Why could not the area of subsidence be changed from the Appalachian region to the ocean on the east? Subsidence in one locality means a corresponding, but not necessarily equal, elevation elsewhere; so while the ocean bed was subsiding may not the Appalachians have risen?

PAGE 57. See the introduction to Palæontology of New York, Vol. III.

PAGE 59. I have shown the existence of a similar condition in Iowa, where the erosion of the Devonian and Silurian surfaces has been covered by coal measures, preserving little evidence of the eroded material. In many instances of this kind deep depressions of considerable area were worn in the older formations, which were filled up by the underclays of the coal measures and often followed by the deposition of the coal above. (See Geological Report of Iowa, pp. 120–141, 1858.)

PAGE 61. Very recently Mr. Gilbert of the U. S. Geological Survey has discovered coal and coal measures in the far west, just enough to make an exception to the general condition here stated.

NOTES ON PROF. JAMES HALL'S ADDRESS.

BY T. STERRY HUNT, LL. D., F. R. S.

The present publication, after more than a quarter of a century has elapsed, of Prof. James Hall's Address before the American Association at the Montreal Meeting in 1857, is eminently fitting at the present time and the views therein expressed will be found for the most part to have lost none of their value, but to have been confirmed by the progress of geological science. It should here be mentioned that most of the important points in this Address were published by Prof. Hall in 1859, in the Introduction to the Third Volume of his great work on the Palæontology of New York, and were the subject of an extended notice by the present writer, in an Essay on Some Points in American Geology, in the American Journal of Science for May, 1861. It was here shown that the views of Prof. Hall, which when heard and read in 1857 and 1859 seemed to many untenable and evoked hostile criticism, were in harmony with those previously maintained by masters in the science of Geology. It is true that the elevation-hypothesis of Von Buch had been extended from volcanic mountains to ranges made up of aqueous sediments, like the Alps, the Pyrenees and the Alleghanies, and by the authority of Humboldt and Elie de Beaumont had been impressed upon the scientific world, but not without protest. Constant Prevost, Scrope and Lyell had opposed this famous hypothesis as applied to volcanic elevations, while de Montlosier in 1832, in the same spirit, declared that the great mountain chains of Europe

are but the remains of continental elevations which have been cut away by denudation and that the foldings and inversions to be met with in the structure of mountains are to be looked upon only as local and accidental.

J. P. Lesley, again, in 1856, had asserted that continents are but congeries of mountains, or rather the latter are but fragments of continents, separated by valleys which represent the absence or removal of mountain-land; and again had said "mountains terminate where the rocks thin out."

Later, the present writer returned to the discussion of Prof. Hall's views in an Essay on Some Points in Dynamical Geology published in the American Journal of Science for April, 1873, and in part a criticism of Professor Joseph LeConte's paper which appeared in the same Journal in November and December, 1872, on the Formation of the Features of the Earth's Crust. I quote from my Essay the following passages, to show how far Hall's views were accepted by LeConte, and also for the reason that in reply to the adverse criticisms of LeConte and others, I have here stated what I conceive to be the great and distinguishing merits of Professor Hall's contribution to the theory of mountains first developed in his remarkable Address of 1857.

"The views of Professor Hall as to the relation between great accumulations of strata and mountain elevations, are cited with approval by LeConte, who, following him, asserts that 'mountain chains are masses of immensely thick sediments.' I venture, however, to remark in this connection that the views both of Mr. Hall, and of myself, as his expounder, have as yet been but imperfectly understood either by LeConte or our other critics. Thus, they have been defined as a theory of mountains with the origin of mountains left out, while LeConte says 'Hall and Hunt leave the sediments just after the whole preparation has been made, but before the actual mountain-formation has taken place.' Now in fact neither Hall nor yet myself in my exposition of his views [in the Essay of 1861 cited above] has proposed any theory to explain this part of the process; that is to say, the uplifting of the deposited sediments, which LeConte calls 'the actual mountain-formation.' Hall's contribution to the problem, which, as LeConte well says, forms 'an era in geological science,' was to show the relation between mountain-chains and great accumulations of sediments; to illustrate this by the history of the palæozoic rocks of North America; and, moreover, to protest against the generally received theory that mountain elevations are due to local upthrusts; he, to use his own language, 'going back to the theories long since entertained by geologists relative to continental elevations.' That mountains were the remains of eroded continental areas had already been taught by Lesley, and long before by de Montlosier. It was left for Hall through a new way to lead us back to these views, but the whole theory of the cause of continental elevation was left by him where he found it. In my exposition of his views, I have only endeavored in addition to show in what manner a contracting globe and a solid nucleus may be related to the great facts of local subsidence and accumulation."

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"Professor LeConte declares that an important problem in geological dynamics remains in his opinion, unsolved, namely the cause of the 'great and widespread *oscillations* which have marked the great divisions of *time*, and have left their impress in the general unconformability of the strata,' the last being that of the post-pliocene period. *Now*, it is precisely the *upward* movements of this kind which constitute the continental elevations of de Montlosier, Hall and myself, giving rise to plateaus, and by the partial erosion and denudation of these, to mountains. The *cause* of these continental elevations was not discussed by Hall, and is by LeConte declared to be unexplained; while such is the case 'the actual mountain formation,' to use his words, is still unaccounted for. That these gentle and widespread movements of oscillation are, however, in some way not yet clearly explained, connected with the contracting of the nucleus, and

the consequent conforming thereto of the envelope, we can scarcely doubt; or that the latter, from its nature and origin, must present great differences in constitution and in flexibility in its various parts. From this it might be expected that the movements imparted to the envelope alike by the process of secular cooling and contraction of the nucleus, and by the disturbance of the equilibrium of pressure consequent on the processes of erosion and sedimentation, would give rise to seemingly irregular oscillations, resulting in the depression or the elevation of considerable areas, constituting continental movements."

The two papers of the present writer will be found reproduced, the first in abstract, in his *Chemical and Geological Essays*, pp. 59–68, and 70–79.

In the latter he has adverted to the view adopted by Hall in his Address, that the crystalline rocks of New England, including the Green Mountains and the White Mountains, are altered palæozoic strata. This view which had been advocated by Mather, the brothers Rogers (though subsequently rejected by them), by Logan and others, was at that time accepted by the present writer; who has, however, long since rejected it as untenable, and maintained the prepalæozoic age of these mountains, and generally of the crystalline rocks of the Atlantic belt, a view which is now shared by most geologists. It is hardly necessary to say that the acceptance or rejection of this hypothesis of the palæozoic age of the crystalline rocks in question in no way affects Professor Hall's argument. The present view of the writer, now maintained by LeConte and most American Geologists, makes the rocks in question a part of the great prepalæozoic continental area lying to the north and east which Professor Hall supposed to be the source of the palæozoic sediments of the Appalachian basin.

SECTION A.

MATHEMATICS AND ASTRONOMY.

ADDRESS

BY

WILLIAM HARKNESS,

VICE PRESIDENT, SECTION A.

MEMBERS OF THE SECTION OF MATHEMATICS AND ASTRONOMY:—

TRANSITS of Venus usually occur in pairs; the two transits of a pair being separated by only eight years, but between the nearest transits of consecutive pairs more than a century elapses. We are now on the eve of the second transit of a pair, after which there will be no other till the twenty-first century of our era has dawned upon the earth, and the June flowers are blooming in 2004. When the last transit season occurred the intellectual world was awakening from the slumber of ages, and that wondrous scientific activity which has led to our present advanced knowledge was just beginning. What will be the state of science when the next transit season arrives God only knows. Not even our children's children will live to take part in the astronomy of that day. As for ourselves, we have to do with the present, and it seems a fitting occasion for noticing briefly the scientific history of past transits, and the plans for observing the one soon to happen.

When the Ptolemaic theory of the solar system was in vogue, astronomers correctly believed Venus and Mercury to be situated between the Earth and the Sun, but as these planets were supposed to shine by their own light, there was no reason to anticipate that they would be visible during a transit, if indeed a transit should occur. Yet, singularly enough, so far back as April, 807,

Mercury is recorded to have been seen as a dark spot upon the face of the Sun. We now know that it is much too small to be visible to the naked eye in that position, and the object observed could have been nothing else than a large sun spot. Upon the establishment of the Copernican theory it was immediately perceived that transits of the inferior planets across the face of the Sun must occur, and the recognition of the value of transits of Venus for determining the solar parallax was not long in following. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks; but Halley was first to work it out completely, and to him is usually assigned the honor of the invention. His paper, published in 1716, was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus of 1761 and 1769, from which our first accurate knowledge of the Sun's distance was obtained.

When Kepler had finished his Rudolphine tables they furnished the means of predicting the places of the planets with some approach to accuracy; and in 1627 he announced that Mercury would cross the face of the Sun on November 7, 1631, and Venus on December 6 of the same year. The intense interest with which Gassendi prepared to observe these transits can be imagined when it is remembered that hitherto no such phenomena had ever greeted mortal eyes. He was destitute of what would now be regarded as the commonest instruments. The invention of telescopes was only twenty years old, and a reasonably good clock had never been constructed. His observatory was situated in Paris, and its appliances were of the most primitive kind. By admitting the solar rays into a darkened room through a small round hole, an image of the Sun nine or ten inches in diameter was obtained upon a white screen. For the measurement of position angles a carefully divided circle was traced upon this screen, and the whole was so arranged that the circle could be made to coincide accurately with the image of the Sun. To determine the times of ingress and egress, an assistant was stationed outside with a large quadrant, and he was instructed to observe the altitude of the sun whenever Gassendi stamped upon the floor. Modern astronomical predictions can be trusted within a minute or two, but so great did the uncertainty of Kepler's tables seem to Gassendi that he began to watch for the expected transit of Mercury two

whole days before the time set for its occurrence. On the 5th of November it rained, and on the 6th clouds covered the sky almost all day. The morning of the 7th broke, and yet there was no respite from the gloomy pall. Gassendi continued his weary watch with the sickening dread that the transit might already be over. A little before eight o'clock the sun began to struggle through the clouds, but mist prevented any satisfactory observation for nearly another hour. Towards nine the sun became distinctly visible, and turning to its image on the screen, the astronomer observed a small black spot upon it. It was not half as large as he expected, and he could not believe it was Mercury. He took it for a sun spot, and carefully estimated its position at nine o'clock, so that he might use it as a point of reference for the planet, if indeed he should be fortunate enough to witness the transit. A little later he was surprised to see the spot had moved. Although the motion was too rapid for an ordinary sun spot, the small size of the object seemed to forbid the idea that it was Mercury. Besides, the predicted time of the transit had not yet arrived. Gassendi was still uncertain respecting the true nature of the phenomenon when the sun again burst through the clouds and it was apparent that the spot was steadily moving from its original position. All doubt vanished, and recognizing that the transit, so patiently watched for, was actually in progress, he stamped upon the floor as a signal for his assistant to note the sun's altitude. That faithless man, whose name has been forgotten by history, had deserted his post, and Gassendi continued his observations alone. Fortunately the assistant returned soon enough to aid in determining the instant of egress, and thus an important addition was made to our knowledge of the motions of the innermost planet of the solar system.

After this success in observing Mercury, Gassendi hoped he might be equally fortunate in observing the transit of Venus on December 6, 1631. He knew that Kepler had assigned a time near sunset for first contact, but the tables were not sufficiently exact to forbid the possibility of the whole transit being visible at Paris. Alas, alas! these hopes were doomed to disappointment. A severe storm of wind and rain prevailed on December 4th and 5th, and although the sun was visible at intervals on the 6th and 7th, not a trace of the planet could be seen. We now know that

the transit happened in the night between the 6th and 7th, and was wholly invisible at Paris.

Transits of Venus can occur only in June and December, and as the two transits of a pair always happen in the same month, if we start from a June transit the intervals between consecutive transits will be 8 years, 105½ years, 8 years, 121½ years, 8 years, 105½ years, and so on. This is the order which exists now, and will continue for many centuries to come, but it is not always so. The path of Venus across the sun is not the same in the two transits of a pair. For a pair of June transits, the path at the second one is sensibly parallel to, and about twenty minutes north of, that at the first; while for a pair of December transits the parallelism still holds, but the path at the second one is about twenty-five minutes south of that at the first. Hence it happens that whenever Venus passes within about four minutes of the sun's centre at a June transit, or within about eight minutes at a December transit, she will pass just outside the sun's disk at the other transit of the pair, and it will fail. Thus the intervals between consecutive transits may be modified in various ways. If the first transit of a June pair fails, they will become 129½ years, 105½ years, 8 years, 129½ years, etc. If the second transit of a June pair fails, they will become 113½ years, 8 years, 121½ years, 113½ years, etc. If the first transit of a December pair fails, they will become 8 years, 113½ years, 121½ years, 8 years, etc. If the second transit of a December pair fails, they will become 8 years, 105½ years, 129½ years, 8 years, etc. And finally, if either the first or second transit of a pair fails both in June and December, they will become 113½ years, 129½ years, 113½ years, 129½ years, etc.

When Kepler predicted the transit of 1631, he found from his tables that at her inferior conjunction on December 4, 1639, Venus would pass just south of the sun, and therefore he believed the second transit of the pair would fail. On the other hand, the tables of the Belgian astronomer, Lansberg, indicated that the northern part of the sun's disk would be traversed by the planet. In the fall of 1639 this discrepancy was investigated by Jeremiah Horrocks, a young curate only twenty years old, living in the obscure village of Hoole, fifteen miles north of Liverpool, and he found, apparently from his own observations, that although

Kepler's tables were far more accurate than Lansberg's, the path of the planet would really be a little north of that assigned by Kepler, and a transit over the southern portion of the sun would occur. He communicated this discovery to his friend William Crabtree, and these two ardent astronomers were the only ones who had the good fortune to witness this, the first recorded transit of Venus.

Horrocks had great confidence in his corrected ephemeris of Venus, and it forbade him to expect the ingress of the planet upon the sun before three o'clock in the afternoon of Sunday, November 24, old style (December 4, new style); but as other astronomers assigned a date some hours earlier, he took the precaution to begin his observations on the 23rd. The 24th seems to have been partially cloudy, but he watched carefully from sunrise to nine o'clock; from a little before ten until noon; and at one o'clock in the afternoon; having been called away in the interval by business of the highest importance—presumably the celebration of divine service. About fifteen minutes past three he was again at liberty, and as the clouds had dispersed, he returned to his telescope and was rejoiced to find Venus upon the sun's disk, second contact having just happened. Only thirty-five minutes remained before sunset, but during these precious moments he made determinations of the position of Venus which are even yet of the highest value. Crabtree was less fortunate. At his station, near Manchester, there was but a momentary break in the clouds a quarter of an hour before sunset. This sufficed to give him a glimpse of the transit, and he afterwards made a sketch from memory.

The years sped swiftly by, and as the transit of 1761 approached, Halley's paper of 1716 was not forgotton, although he himself had long been gathered to his fathers. In deciding to what extent his plans could be followed, it was first of all necessary to know how nearly the real conditions would approximate to those he had anticipated. Passing over a paper by Trébuchet calling attention to errors in Halley's data, Delisle was first to point out the exact conditions of the transit, and the circumstances upon which the success of the observations would depend. In August, 1760, less than a year before the event, he published a chart showing that inaccurate tables of Venus had misled Halley, both as to the availability of his method, and in the selection of stations. The occasion could be more effectively utilized by a change of plan,

and Delisle considered it best to observe at suitably selected localities from many of which only the ingress, or only the egress, would be visible. Ferguson, in England, seems to have arrived independently at similar conclusions.

The two methods proposed respectively by Halley and Delisle have played so important a part in the history of physical astronomy that it will not be amiss to state briefly the distinction between them. The sun causes Venus to cast a shadow which has the form of a gigantic cone, its apex resting upon the planet, and its diameter continually increasing as it recedes into space. All the phenomena of transits are produced by the passage of this shadow cone over the earth, and as each point of the cone corresponds to a particular phase of a transit, any given phase will encounter the earth, and first become visible, at some point where the sun is just setting; and will leave the earth, and therefore be last visible, at some point where the sun is just rising. Between these two points it will traverse nearly half the earth's circumference and in so doing will consume about twenty minutes. The only phases dealt with by either Halley's or Delisle's method are the external and internal contacts, both at ingress and at egress. Delisle's method consists in observing the times of contact at stations grouped about the regions where either ingress or egress is soonest and latest visible. The longitudes of the stations must be well determined, and then by combining them with the observed times of contact the rate at which the shadow cone sweeps over the earth becomes known, and from it the solar parallax results. At many of the stations best suited for Delisle's method, only the beginning or only the ending of the transit will be visible; but for the application of Halley's method, both the beginning and the ending must be seen. The theory of the latter method is so complicated that it is difficult to explain it briefly and at the same time accurately; but the following considerations will suffice to indicate its nature. The duration of a transit at any point on the earth's surface depends partly upon the length of path, and partly upon the velocity, of that point while within the shadow cone. The length of path is affected by the latitude of the point, and the velocity by the earth's diurnal motion, which in some regions accelerates, and in others retards, the progress of the shadow. The result is that throughout one-half the earth's surface the duration of the transit is lengthened, while throughout the other

half it is shortened; the maximum lengthening and shortening occurring at the respective poles of the hemispheres in question. Although these poles are not situated at the extremities of the earth's axis, it usually happens that one of them is shrouded in night; but upon the sunlit side of the earth, from which alone observations can be made, localities may exist at some of which the duration of the transit will be twenty minutes or more greater than at others. This inequality is the condition upon which Halley's method depends, and when such localities are accessible it may be advantageously applied. Briefly then, Halley's method consists in observing the duration of a transit at two or more stations so selected as to give durations of widely different lengths; while Delisle's method consists in employing a common standard time to note the instant when the transit begins, or ends, at two or more stations so chosen as to give very different values for that instant.

The transit of 1761 was visible throughout Europe and was well observed by astronomers in all parts of that continent. Besides this, England sent expeditions to St. Helena and to the Cape of Good Hope; and English astronomers observed at Madras and Calcutta; French astronomers were sent to Tobolsk, Rodriguez, and Pondicherry; Russians to the confines of Tartary and China; and Swedes to Lapland. No less than 117 stations were occupied by 176 observers; and of these, 137 published their observations. When this mass of data was submitted to computation, the result was far from satisfactory. Values of the solar parallax were obtained ranging from 8.49 seconds to 10.10 seconds; and in their disappointment the astronomers of the eighteenth century concluded that too much reliance had been placed upon Delisle's method.

The transit of 1769 drew on apace; and, to avoid a repetition of the fancied mistake of 1761, attention was directed almost exclusively to Halley's method. The conditions of the transit were carefully discussed by Hornsby in England, and by Lalande and Pingré in France; and it was found that its duration would be greatest in Lapland and Kamschatka, and least in the Pacific Ocean, California and Mexico. Astronomers were dispatched to all these regions. England sent the famous Capt. Cook to Otaheite; France sent Chappe to California; the King of Denmark sent Father Hell to Lapland; and in addition numerous observations were made in Europe, North America, China, and the East Indies.

The preparations were most elaborate, and the result better than in 1761, but still not satisfactory. The black drop and other distortions disturbed the contacts in this transit as they had done in the previous one, and the values of the parallax deduced by the best computers ranged from 8.43 seconds to 8.85 seconds.

Thus the matter rested till 1825 and 1827 when Encke published abstracts of his discussion of the transits of 1761 and 1769, from which he deduced a parallax of 8.58 seconds. This discussion was not printed in full till 1835, when it immediately commanded the attention of astronomers, and its result, which Encke had modified to 8.57 seconds, was universally accepted for more than a quarter of a century. As time wore on, certain gravitational investigations led to a strong suspicion that the sun's distance had been overestimated by at least three million miles, and the observations of Mars at its opposition in 1862 converted this suspicion into a conviction. The eighteenth century transits were again rediscussed and a parallax of 8.83 seconds was found from them by Powalky in 1865, and 8.91 seconds by Mr. E. J. Stone in 1868. Newcomb's paper, in 1867, also produced a marked impression.

The transit of 1874 was then approaching, and in the discussion as to how it should be utilized Halley's and Delisle's methods once more played a prominent part. It was recognized that the uncertainty in the observed times of contact of the eighteenth century transits was largely due to the black drop, and the causes of that phenomenon were carefully considered. Among them, most astronomers believed that irradiation played an important, if not the principal, part; but at the same time there was a general feeling that the telescopes of a century ago were bad, and that the magnificent instruments of the present day would give better results. In view of all the circumstances it was determined that the contacts should be observed with equatorially-mounted achromatic telescopes of from 4 to 6 inches' aperture, or with reflectors of not less than 7 inches' aperture, and that magnifying powers of from 150 to 200 diameters should be employed. The Germans and Russians adopted heliometers of about three inches' aperture for making exact determinations of the positions of Venus during transit, but other nations did not follow their example.

Photography, an agency undreamed of in the eighteenth century, was also available, and all saw the desirability of employing it; but there was much difference of opinion as to how this should be

done. The European astronomers preferred instruments modelled upon the Kew photoheliograph, whose objective has 3.4 inches aperture and 50 inches focus, giving an image of the sun 0.482 of an inch in diameter, which is enlarged by a secondary magnifier to 3.93 inches. On the other hand, the American astronomers contended that photographs taken with such instruments would be affected by troublesome errors due to the secondary magnifier, that position angles could not be measured from them accurately enough to be of any use, and that it would be exceedingly difficult to determine the exact linear value of a second of arc. They advocated the use of horizontal photoheliographs, which are free from all these disadvantages; and the instruments which they adopted had apertures of 5 inches, and focal distances of $38\frac{1}{2}$ feet, giving images of the sun slightly more than 4 inches in diameter. Notwithstanding this radical difference of opinion respecting the best form of photoheliograph, the astronomers of the old and new worlds were in perfect accord as to how the instruments should be employed. Between the first and second contacts, and again between the third and fourth contacts, photographs about five minutes square, showing the indentation cut by the planet into the sun's limb, were to be taken at intervals of a few seconds; and from these it was hoped the true times of contact could be deduced with great accuracy. Between the second and third contacts, pictures of the entire sun were to be taken at short intervals, and the positions of Venus relatively to the sun's centre were to be obtained from them by subsequent measurements. In the latter case, the photoheliograph took the place of a heliometer, and was superior to that instrument in its power of rapidly accumulating data.

The question of instrumental outfit having been disposed of, stations were selected, and parties dispatched to almost every available point. The United States, England, France, Germany, Russia, Holland,— in short, nearly all the nations of the civilized world,— took part in the operations. The weather was not altogether propitious on the day of the transit, but nevertheless a mass of data was accumulated which will require years for its thorough discussion. When the parties returned home the contact observations were first attacked, but it was soon found that they were little better than those of the eighteenth century. The black drop, and the atmospheres of Venus and the Earth, had again produced a series of complicated phenomena, extending over many

seconds of time, from among which it was extremely difficult to pick out the true contact. It was uncertain whether or not different observers had really recorded the same phase, and in every case that question had to be decided before the observations could be used. Thus it came about that within certain rather wide limits the resulting parallax was unavoidably dependent upon the judgment of the computer, and to that extent was mere guesswork. Attention was next directed to the photographs, and soon it began to be whispered about that those taken by European astronomers were a failure. Even yet I am not aware that the Germans have published anything official on the subject; but the English official report has appeared, and it frankly declares that "after laborious measures and calculations it was thought best to abstain from publishing the results of the photographic measures as comparable with those deduced from telescopic view." From the way in which these photographs were taken, Sir George Airy saw that they could not yield position angles of any value, and therefore differences of right ascension and declination could not be determined from them; but they did seem capable of giving the distance between the centres of Venus and the sun with considerable accuracy. Upon trial this proved not to be the case. No two persons could measure them alike, because "however well the sun's limb on the photograph appeared to the naked eye to be defined, yet on applying to it a microscope it became indistinct and untraceable, and when the sharp wire of the micrometer was placed on it, it entirely disappeared." In short, the British photographs are useless for the present, but Sir George Airy hopes that in the future some astronomer may be found who will be capable of dealing with them.

We turn now to the American photographs. They present a well defined image of the sun about 4.4 inches in diameter, and are intended to give both the position angle and distance of Venus from the sun's centre. A special engine was at hand for measuring them, but when they were placed under the microscope only an indistinct blur could be seen. Here again was the same difficulty which had baffled the English, but fortunately its cause was soon discovered. The magnifying power of the microscope was only 37½ diameters, which seemed moderate enough, but was it really so? The photographic image of the sun was about 4.4 inches in diameter, and this was magnified 3.31 times by the objective of the microscope, thus giving an image 14.56 inches in diameter.

To yield an image of the same size, a telescopic objective would require a focus of about 1563 inches, and if the eye-piece of the microscope, which had an equivalent focus of 0.886 of an inch, were applied to it, a power of 1764 diameters would be produced. This then was the utterly preposterous power under which the image of the sun was seen when the photograph was viewed through the microscope,and no useful result could be expected from it. Means were immediately provided for reducing the power of the microscope to 5.41 diameters, and then the photograph seen through it appeared as the sun does when viewed through a telescope magnifying 255 diameters. After this change all difficulty vanished, and the photographs yielded excellent results. The measurements made upon them seem free from both constant and systematic errors, and the probable accidental error of a position of Venus depending upon two sets of readings made upon a single photograph is only 0.553 of a second of arc. To prevent misunderstanding it should be remarked that this statement applies only to pictures taken between second and third contact, and showing the entire sun. The small photographs taken between first and second contact, and again between third and fourth contact, proved of no value.

These investigations consumed much time, and before the result from the American photographs was generally known, an international convention of astronomers was held in Paris to consider how the transit of 1882 should be observed. The United States was not represented at this conference, and guided only by their own experience, the European astronomers declared that photography was a failure and should not be tried again. They knew that the contact methods are attended by difficulties which have hitherto proved insurmountable, but under the merciless pressure of necessity, they decided to try them once more. Unfettered by the action of the Paris Conference, the United States Transit of Venus Commission took a very different view of the case. Its members knew that the probable error of a contact observation is 0.15 of a second of arc, that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. They also knew that the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only 0.55 of a second of arc. If

the sun is visible for so much as six minutes between the second and third contacts, by using dry plates thirty-six negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. These were the reasons which led the American Commission to regard photography as the most hopeful means of observation, and thus it happens that the astronomers of the old and new worlds differ radically respecting the best means of utilizing one of the most important astronomical events of the century. The Europeans condemn photography, and trust only to contacts and heliometers; the Americans observe contacts because it costs nothing to do so, but look to photography for the most valuable results.

In 1716, Halley thought that by the application of his method to the transit of 1761, the solar parallax could certainly be determined within the five hundredth part of its whole amount. Since then, three transits have come and gone, and the contact methods have failed to give half that accuracy. From the photographic method, as developed by the U. S. Transit of Venus Commission, we hope better things, and perhaps fifty years hence its results may be regarded as the most valuable of the present transit season. In 1874, as in 1761, exaggerated views prevailed respecting the value of transits of Venus, but no competent authority now supposes that the solar parallax can be settled by them alone. The masses of the Earth and Moon, the moon's parallactic inequality, the lunar equation of the earth, the constants of nutation and aberration, the velocity of light, and the light equation, must all be taken into account in determining the solar parallax, and it cannot be regarded as exactly known until the results obtained from trigonometrical, gravitational, and photo-tachymetrical, methods are in perfect harmony. It may be many years before this is attained, but meanwhile practical astronomy is not suffering. Its use of the solar parallax is mainly confined to the reduction of observations made at the surface of the earth to what they would have been if made at the Earth's centre; and for that, our present knowledge suffices. The real argument for expending so much money upon transits of Venus is that being an important factor in determining the solar parallax, their extreme rarity renders it unpardonable to neglect any opportunity of observing them. Let us do our whole duty in this matter that posterity may benefit by it, even as we have benefited by the labors of our predecessors.

PAPERS READ.

New views of Mr. George H. Darwin's Theory of the Evolution of the Earth-Moon System, considered as to its bearing on the question of the duration of Geological Time.

By Samuel Haughton, of Dublin, Ireland.

It has been tacitly assumed, even so far back as the times of Newton and Clairaut, that the earth and planets have passed through a liquid condition (owing to former great heat) before assuming the solid condition, which some of them, at least, now possess.

Laplace, in his nebular hypothesis, also assumes the former existence of this liquid condition, and it is openly asserted by all geologists who believe that the earth consists of a solid crust (more or less thick), reposing upon a fluid or viscous nucleus.

It has been proved by Sir William Thomson, following out the views of the late Mr. Hopkins, that the present condition of the earth, taken as a whole, is such that it must be regarded as being more rigid than glass or steel, possibly more rigid than any terrestrial substance under the surface conditions of pressure.

The following considerations show that it may be fairly doubted whether the earth or any other planet ever existed in a fluid condition.

1. The possibility of the equilibrium of the rings of Saturn, on the supposition that they are either solid or liquid, has been more than doubted, and the most probable hypothesis respecting them is, that they consist of swarms of discrete meteoric stones.

2. It is difficult to understand the low specific gravity of Jupiter and the other outer planets, on the supposition that they are either solid or liquid, for we know of no substance light enough to form them.[1] If the outer planets consist of discrete meteoric

[1] The force of this argument could not be felt before the revelations of the spectroscope, because at that time there was no proof that the whole universe was composed of the same simple substances, and those very limited in number.

stones moving around a solid or liquid nucleus, the difficulty respecting their specific gravity would disappear.

3. The recent researches connecting the November, the August, and other periodic swarms of shooting stars with comets tend in the direction of showing that comets, in cooling, break up into discrete solid particles (each no doubt having passed through the liquid condition); and that probably the solar nebula cooled in like manner into separate fiery tears, which soon solidified by radiation into the cold of space.

4. Mr. Huggins's recent comparisons of the spectroscopic appearances of comets and incandescent portions of meteoric stones, showing the presence in both of hydrocarbon and nitrogen compounds, confirm the conclusions drawn from the identity of the paths of comets and meteoric periodic shooting stars.

5. Mr. H. A. Newton, in a remarkable paper read before the Sheffield Meeting of the British Association (1879), showed the possibility (if not probability) of the asteroids being extinct comets, captured and brought into the solar system by the attraction of some one or other of the outer large planets, and permanently confined in the space between Mars and Jupiter, which is the only prison cell in the solar system large enough to hold permanently such disorderly wanderers.

In the same paper, Professor Newton threw out the idea that some of the satellites of the large planets might also be of cometary origin.

From all these and other considerations, it is therefore allowable to suppose that the earth and moon, when they separated from the solar nebula, did so as a swarm of solid meteoric stones, each of them having the temperature of interstellar space; *i. e.*, something not much warmer than 460° F. below the freezing point of water.

Mr. George H. Darwin has shown, admirably, how the earth-moon system may have been developed from the time when the earth-moon formed one planet revolving on its axis in a few hours, to the present time, when the earth and moon (in consequence of tidal friction) have pushed each other asunder to a distance of sixty times the radius of the earth.[2]

In his paper on the tidal friction of a planet[3] (supposed viscous and under the influence of bodily tides caused in it by an external

[2] Proceedings of Royal Society, 19th June, 1879.
[3] Phil. Trans., 1881, Part ii, p. 494.

body such as the moon), Mr. Darwin has found a remarkable equation of condition, which may be thus expressed:

$$d(\sqrt{r}) \propto \frac{\Psi dt}{r^6} \tag{1}$$

where r = distance between centres of earth and moon.
t = time elapsed from a fixed epoch.

$$\Psi = \frac{p(n-\Omega)}{1+p^2(n-\Omega)^2} \tag{2}$$

n = angular velocity of earth's rotation.
Ω = angular velocity of moon's orbital revolution.
p = quantity varying inversely as the viscosity of the planet.

The extreme interest of equation (1) consists in the appearance of the inverse sixth power of the distance.

As the function Ψ varies very slowly, we find by integration, for any portion of time during which Ψ may be regarded as constant

$$t = Ar^{\frac{13}{2}} + B, \tag{3}$$

a most unexpected and remarkable result.

Upon reading Mr. Darwin's papers, my mind turned to a problem with which I was familiar, viz.: the retardation of the earth's rotation produced by the lunisolar tide exerted upon the ocean supposed collected in an equatorial canal, the moon and sun having no declination, and I readily found an equation to express the evolution of the earth-moon system, on the foregoing hypothesis as to friction.

This equation is the following:

$$d(\sqrt{r}) \propto \frac{f\Phi dt}{r^6} \tag{4}$$

where

$$\Phi = \frac{V_0(n-\Omega)}{\{4V_0^2(n-\Omega)^2 - k^2\}\sqrt{4(n-\Omega)^2+f^2}} \tag{5}$$

f = coëfficient of friction supposed proportional to relative velocity.
k varies inversely as r^3.
V_0 = velocity at earth's equator.

This leads, as in Mr. Darwin's hypothesis of viscous earth, to the integral

$$t = A'r^{\frac{13}{2}} + B' \tag{6}$$

The form of the functions Ψ and Φ is similar, as both ascend by odd powers of $(n-\Omega)$ and vanish when $n=\Omega$, that is to say, at the beginning and end of the evolution by friction of the earth-moon system.

It is quite clear, therefore, that the remarkable expression (1) found by Mr. Darwin, is not peculiar to his special hypothesis of a viscous earth, but can be deduced equally well from the totally distinct hypothesis of an absolutely rigid earth retarded by the tidal action of a liquid ocean.

I was led by this result to consider the case of the earth-moon separating (as I believe they did) from the central solar mass, in the form of a swarm of discrete masses of meteoric iron and stone, each one having the temperature of the cold of interstellar space, or not much above it. Translating this conception into mathematical language, I find that the equation of continuity belonging to the hydrodynamical theory applies equally well to the meteoric theory, viz.:

$$vy=v'y' \qquad (7)$$

where v, v' are the velocities at any two points, and y, y' are the depths of the ocean or meteoric swarm at the same points.

The depth of the swarm or ocean, without jostling or friction will be least under the moon, and greatest at right angles to the moon, and the velocities will be inversely. Hence the chances of jostling among the meteorites, when disturbed by the moon's tidal action will be proportional to the velocity, being greatest where the velocity is greatest and the area of passage least, and *vice versa*.

This consideration reduces the meteoric problem to that of the hydrodynamical problem, with a friction proportional to the velocity, and gives equations, in all respects similar to those derived by Mr. Darwin, from the hypothesis of a viscous earth.

On the meteoric hypothesis, if the jostling of the stones be slow they may cool almost as fast as they are heated and the result will be a cool earth and almost indefinite time at the disposal of geologists.

THE PARALLAX OF α LYRÆ AND 61 CYGNI. By ASAPH HALL, of Washington, D. C.

THE question of the parallaxes of the fixed stars, or their distances from the earth, was for a long time a subject of speculation among the ancient astronomers. Very little, however, could be done towards answering this question until the true theory of our solar system had been discovered, and the invention of the telescope had made possible more accurate astronomical observations. Even when these things had been done there remained other questions to be solved, such as those of aberration and refraction, before astronomers could reduce their observations correctly and bring the problem of stellar parallax to the test of exact measurement. All these reductions are easy to us now, but the astronomer who lived two hundred years ago found himself in a maze of perplexities, and nearly a century passed away before these difficulties were overcome so far as to make feasible the determination of the distances of the stars.

When it was seen that the motion of the planets around the sun is undoubtedly the true theory of our solar system, and that we have a long base line to measure from, astronomers were not slow to undertake observations for stellar parallax. One of the earliest observers was Rothmann, astronomer for William of Hesse, in Germany. Rothmann found a difference of 2′ in the latitude of his observatory from observations made in summer and in winter, and hence he inferred that the stars have an annual parallax, which confirmed the Copernican theory that the earth is in motion around the sun. But Tycho Brahe, the best observer of those times, declared that his own observations, made with the best instruments and with the greatest care, showed no such change of latitude; and hence Tycho drew the conclusion that the stars have no parallax, and that the theory of the motion of the earth around the sun is false. One of the most ingenious early attempts to investigate stellar parallax was that of Dr. Robert Hooke of London in 1669; but Hooke seems to have been a man of unsteady purpose, and failed to carry out his plan of observation, so that his work came to nothing. The next remarkable attempt was by the English astronomer, Flamsteed, in 1689–1697. Flamsteed made very careful determinations of the zenith distances of Polaris at its upper and lower culminations at different

times of the year, and correcting these for precession he found that the results showed a periodical change, which he ascribed to parallax. But as soon as Flamsteed published his observations, it was pointed out by Cassini and others that the periodical changes could not be explained by a parallax of the star. It is curious, however, that no serious attempt was made to account for Flamsteed's results, the most careful and accurate that had been obtained up to his time. These results contain, in fact, the data for a very good determination of the constant of aberration, as was shown by C. A. F. Peters in 1846, and the value of this constant found from Flamsteed's observations is in error only a quarter of a second of arc.

Another generation of astronomers continued their attempts at the determination of stellar parallax, from observations of the zenith distances of the stars, but with no results. Many of these observations show periodical changes, which gave rise to various theories; but probably all these changes may be explained by variations in the instruments, or by incomplete reduction of the observations. In fact, no trustworthy determinations of parallax were possible until after the discovery of the aberration of light; and Bradley's discovery of this phenomenon, in 1728, put an end to most of the crude attempts. The coëfficient of aberration is 20.″4; and although the correction for aberration is zero at the time of the maximum of parallax, yet the motion of the earth in a single day will introduce a correction for aberration amounting to one-third of a second of arc, a quantity comparable with the parallaxes of the stars. Bradley was one of the few astronomers of the past century who knew the value of accurate and definite observations, and from his own work he drew the conclusion that the parallaxes of the stars must be less than a second of arc. Bradley's labors seem to have shown astronomers the great practical difficulty of this problem of stellar parallax, and few further attempts were made until toward the beginning of the present century when the instruments had been much improved.

The Italian astronomer, Piazzi, in 1792–1804, found parallaxes of several of the bright stars, amounting in some cases to 3″ or 4″, but his results are certainly wrong. About this time Brinkley, the astronomer Royal of Ireland, devoted fourteen years of careful and intelligent observation to the question of stellar parallax. This is one of the most remarkable researches of the kind ever

made. The values of parallax found by Brinkley amount in some cases to 2″; and his work led to a long dispute between himself and Pond, the astronomer Royal at Greenwich. Although Brinkley was a skilful astronomer, and a man whom no amount of labor could deter, yet we must say that his results for parallax are mostly erroneous. However, the great difficulty of dealing with this question, by means of absolute measures made with divided circles, seems to have been brought out clearly by Brinkley's observations and their subsequent criticism and discussion.

The most elaborate and successful attempt that I know of to determine the parallaxes of the stars by such absolute measurements is that made by C. A. F. Peters at Pulkowa in 1842–'43. In the care and skill of observation, and completeness of reduction, this work has never been surpassed. Peters finds positive values of the parallaxes of seven of the eight stars observed, and in the case of the single negative parallax the value is less than a tenth of a second. The parallax of 1830 Groombridge found by Peters is $0''.226 \pm 0''.141$; a value much less than that found by a differential method, and subsequent investigations have proved the correctness of Peters' result.

The absolute determination of so small a quantity as a stellar parallax is an extremely difficult matter, since the observations must be extended over a year, during which the instrument is exposed to great changes of temperature which act on the circle, and which may influence the reductions. The divided circle may be avoided by using a transit instrument in the prime vertical, although the field of observation is in this way restricted to fewer stars. If Struve's method of reversing the instrument in each vertical be used, and stars are chosen that pass near the zenith, two of the instrumental corrections are almost wholly eliminated; but the third, or level correction, comes in with nearly its full value. With the excellent spirit levels made by Repsold, it was found by Struve that the probable error from this source was only a few hundredths of a second of arc. For stars of the first and second magnitudes that can be observed at all seasons of the year, this instrument promises good results, and in the absolute determinations of parallax, and in the investigations of the constants of aberration and nutation, it may yet fulfil the expectations of Struve.

It was long ago pointed out by Galileo, that if two stars are near

each other, and one of them is bright and the other faint, probably these stars are at greatly different distances from us; and that differential observations of such stars would lead to a knowledge of their difference of parallax. Huygens applied this method to some of the binary systems, and of course found no parallax. A century afterwards Herschel also applied this method to some of the binary stars, and again no parallax was found; but by continuing his observations Herschel found that many of these stars were in motion around each other, and recognizing that such stars must be at nearly the same distance from us, he saw that no parallax could be found from such observations. In some respects, however, this method of Galileo presents great advantages. The observations being merely differential they can be made with micrometers of various kinds, and with greater accuracy and ease than absolute determinations. The reductions are also far simpler and more easily computed, and we avoid in a good degree the effects produced by changes of temperature. It is true that by this method we get only the difference of the parallaxes of the stars, and there will always remain some doubt concerning the value of the real parallax. But if a number of different stars be used for comparison, and the same value of the parallax be found from the various stars, we may fairly conclude that the parallax belongs to the principal star. A practical difficulty is met with here in the fact that most of the stars that have large proper motions, and those that for other reasons are such as we would observe for parallax, have no suitable comparison stars within reach of the micrometers commonly in use; that is, if we follow the method of measuring angles of position and distance, which is perhaps the best. In some cases this difficulty may be avoided by observing only the difference of declination, when the coëfficient of parallax in declination is not too small, although by doing this we give up the advantage of an independent determination of the parallax from the angle of position.

In 1862 a series of observations of α Lyræ was begun at the U. S. Naval Observatory by Professor J. S. Hubbard, and this series was continued by Professor S. Newcomb and his assistants until April, 1867. Each complete observation consists of four transits over seven wires, the instrument being reversed in each vertical. This star passing only a quarter of a degree south of our zenith, the correction for azimuth is nearly zero, and the colli-

mation being eliminated by the method of observing, there remained only the correction for the level, which was determined by an excellent instrument made by Würdemann of Washington. This long and careful series of observations proved, however, of no value for determining the parallax of this star. Some unknown disturbance acted in such a way that the parallax comes out negative. This unfortunate result led me to make an attempt to find the parallax of this star by the differential method, while our observatory remains on its present site. These observations were begun May 24, 1880, and were finished July 2, 1881. As the famous star 61 Cygni could be observed without spending much more time, observations on this star were begun Oct. 24, 1880, and ended Dec. 7, 1881. These observations were the simplest possible, being measures of the difference of declination from a comparison star of the tenth magnitude. My observations of α Lyræ consist of two series, one with a bright field and dark wires, and the other with a dark field and bright wires; those of 61 Cygni were made with the dark wires only. Before beginning these measurements an arrangement was attached to the tube of the micrometer for adjusting the stellar focus, so that this adjustment could be deliberately and carefully made, and during the work care was taken that this adjustment should be always exact. In the case of our 26-inch refractor the tube of the micrometer is shifted one-thirtieth of an inch during the year, it being pushed in during the cold weather and drawn out in summer. It has been assumed in all our previous reductions that the coëfficient of temperature for the screw of our micrometer is zero. To test this assumption, which would have an important influence on the reductions, at the same time that the observations for parallax were going on a series of measurements was made on a pair of stars in the cluster h Persei. These measurements were not reduced until the observations for parallax were finished, and on account of the previous result of a zero coëfficient for temperature, the measurements were not so numerous as they might otherwise have been; still they show a decided correction for change of temperature.

The observations for parallax were arranged so that the micrometer and the wires were reversed on each night, and at the end of an observation the wires were restored to their first position. Each of these observations being reduced to a chosen epoch by

correcting for proper motion, precession, nutation and aberration, gives an equation of condition of the form

$$x + by + cz + du + n = o,$$

where x is the correction to the assumed difference of declination, y the correction to the assumed value of the annual proper motion, z the difference of the constants of aberration for the two stars, u the difference of the parallaxes of the stars, and n is the residual between the observed difference of declination and an assumed value. The assignment of weights to such equations is a delicate matter, and whenever possible I have preferred to give the weight unity to each of them, notwithstanding the fact that the observations were made under very different conditions of the images. The solution of each set of equations by the method of least squares gives the following values of the parallax:

α Lyræ.

Dark wires, $\pi = 0''.1556 \pm 0''.00764$.
Bright " $\pi = 0.2080 \pm 0\ .00827$.

Taking the mean of these results by weight, we have the final result;

$\pi = 0''.1797 \pm 0''.00561$.
Time of light = 18.11 Julian years.

61 Cygni.

Dark wires, $\pi = 0''.4783 \pm 0''.01381$.
Time of light = 6.803 Julian years.

The substitution of these values in the equations of condition shows that the greatest residual in the case of α Lyræ is for the dark wires $0''.120$; and for the bright wires it is $0''.160$, there being 69 equations in the first case and 59 in the second. The observations of 61 Cygni are not quite so good, the greatest residual here among the 66 equations being $0''.209$.

The star 61 Cygni has one of the largest and best determined parallaxes that we know of, since a recent investigation by Dr. Elkin has reduced that of α Centauri to nearly the same value. This quantity being half a second of arc, it can be seen easily with a good telescope. But a tenth of a second is so small that one

can hardly distinguish it even with the highest magnifying powers, except with remarkably steady and distinct images. To determine a parallax of this amount we must trust therefore that in a large series of measurements the influence of the parallax will be shown in the mean result. And my own experience has convinced me that such will be the case, if the observer be careful, and has practised so long that his habit of observing will not change during the period of observation.

On a method of reducing different Catalogues of Stars to a homogeneous system. By William A. Rogers, of Cambridge, Mass.

[ABSTRACT.]

When two catalogues of stars which have been independently observed are compared, it will be found that there are certain systematic deviations *inter se* which are functions either of the right ascension or of the declination, or of both the right ascension and the declination. If all the stars in the two catalogues are compared for each hour of right ascension and the mean of the deviations for each hour be taken, it will be found that this mean is never a constant. So also the mean of the residuals obtained by combining the stars in groups, arranged in the order of declination, will be found to vary with the declination.

Now our knowledge of the motion of the solar system in space depends upon our knowledge of the proper motions of the stars composing the stellar system. Since all the catalogues of stars which have ever been made, have variations from any normal system assumed, it is necessary to reduce each catalogue to a common system before the observations can be employed in the determination of the proper motion.

It is the common practice to arrange the stars in the order of right ascension and to take the mean of the residuals from the normal system chosen for each hour. Since these residuals involve the accidental errors of observation, it is then necessary to obtain a system of mean corrections which shall be as nearly

free as possible from accidental errors. There are two methods of doing this—

I. The Analytical Method.

If we have a series of residuals $\Delta\alpha$ arranged in the order of right ascension, which are strictly circular functions, we may always assume:

$$\Delta\alpha = \text{a constant} + m \sin \alpha + n \cos \alpha + m' \sin 2\alpha + n' \cos 2\alpha \text{ etc.}$$

From the solution of a series of equations of this form the values of m n m' and n' can be found by the process of least squares. Hence the value of $\Delta\alpha$ becomes known for any right ascension.

II. The Graphical Method.

If we assume any given unit in right ascension as a horizontal argument, and an aliquot part of $\Delta\alpha$ as a vertical argument, it is obvious that points representing $\Delta\alpha$ may be laid off which will bear a definite relation to a fixed horizontal line. If a smooth curve be drawn through these points values of $\Delta\alpha$ nearly representing observation may be derived for any right ascension by reading off the vertical coördinate passing through the curve at this point.

Now when the systematic deviations of any catalogue from the normal system have been found by either of these methods and have been applied to the positions of the catalogue, it will still nearly always be found that there are remaining residuals which are functions of the declination. These residuals can be treated either analytically or graphically by the methods just described.

But when the chosen catalogue has been reduced to the normal system by the application of both of these classes of systematic corrections, *i. e.*, those depending on the right ascension, and those depending on the declination, obtained independently, there are several catalogues which still show a system of residuals which are functions of both the right ascension and of the declination.

It is easy to see why this must be so, to a certain extent. Nearly all modern catalogues depend upon Bessel's constants of precession. A few, however, depend upon Struve's constants.

In order to reduce the annual variation for Bessel's constants to those of Struve, we have:—

$$\text{Struve} = \text{Bessel} + .001123^{s} + 000129^{s} - \text{tang } \delta \sin \alpha$$

It will be seen that the variable part of this reduction depends upon both α and δ.

Both of the methods described for comparing different catalogues with a normal catalogue are open to the objection that the coördinates of any given star when referred in this way to a normal system, may differ by a small amount from the values of the coördinates derived by a direct reduction of the instrumental constants from the fundamental stars of the system. I estimate the uncertainty arising in this way to be about $\pm 0.015^s$ in $\Delta\alpha$ and $\pm 0.20''$ in $\Delta\delta$.

In the reduction of the zone stars observed at the Harvard College Observatory, the instrumental constants from 1871 to 1876 have been recomputed from the final positions of the fundamental catalogue upon which it is to be based, viz., upon the catalogue of fundamental stars in publication, the primary reductions having been made with the provisional places of these stars. When, therefore, the final catalogue is completed, the data will be at hand for a definite determination of the amount of this uncertainty. But since this reduction is entirely impracticable in the case of the catalogues already published finally, some method of reducing different catalogues to a homogenous system is an absolute necessity. Admitting the necessity — *that method is to be preferred which will reduce the residuals $\Delta\alpha$ and $\Delta\delta$ to a minimum, whatever the order or the limits of the groups into which they may be divided.*

The method proposed is as follows: —

(*a*) The residuals are first arranged in the order of declination. The mean values for any group represent nearly the corrections for the mean declination of that group, and for twelve hours of right ascension.

(*b*) The residuals for each group minus the mean value for that group, are then arranged in the order of right ascension, and a graphic curve is drawn through the points representing these values. Each curve will give a series of values for the even hours of right ascension, which are arranged in vertical columns, the horizontal argument being the mean declination. It will be seen that the values in the vertical columns are derived from the same curve, while those in the horizontal columns are derived from different curves. In order to connect the different groups in declination a curve is drawn through the points represented by the residuals in the horizontal columns. This will, of course, disturb to a slight

extent, the values already found in the vertical columns, but they can in turn be rectified in the way already described. A second approximation will ordinarily give smooth curves for both the vertical and the horizontal arguments.

(c) On account of the limited number of residuals which usually compose the groups arranged in the order of declination, there is danger of introducing systematic errors in drawing the graphic curves. Hence, after the sum of the two residuals already found has been subtracted from the original residuals, the new values are arranged in the order of right ascension and a smooth curve is drawn through the points thus found, giving the means of obtaining the slight corrections which still remain.

This method has been followed in a paper printed in Vol. X of the Memoirs of the American Academy of Arts and Sciences on "A comparison of the Harvard College Observatory Catalogue of Stars for 1875.0 with the fundamental systems of Anwers, Safford, Boss and Newcomb."

On the Performance of a New Form of Level invented by Mr. John Clark of the U. S. Coast Survey. By William A. Rogers, of Cambridge, Mass.

[ABSTRACT.]

Experienced observers have often expressed the opinion that the level as ordinarily constructed cannot be regarded as an instrument of precision. It will be admitted on every hand that it is not made by a scientific method. In fact, the method of construction is of the most crude kind. The maker chooses a glass cylinder as uniform in thickness and density as he can obtain and proceeds to grind an interior curve of unknown radius with an emery and a polishing tool. He has no means of determining the exact radius by observation. He is unable to test the equality of the curvature on each side of the centre, until after the cylinder has been filled with a fluid and after the ends have been hermetically sealed. Even then the test is only a relative one. It is of the

same kind as the observations which are made with the completed level.

As the result of many years of experience I have reached the conclusion that an astronomical level can be regarded as an instrument of precision, only when it is subjected to adequate tests during the series of observations in which it is employed. A given level may measure small angles with great precision, but the same level under different conditions may give untrustworthy results.

Even with an instrument which has no graduated circle, an adequate level trier can be constructed by placing a graduated scale at a known focal distance. In the case of the Meridian Circle of Harvard College Observatory the level is placed in Y's attached to the cube of the instrument placed parallel with the optical axis.

In order to show the necessity for repeated and continuous observations for the determination of the value of one division of the level, I record here my experience with the companion level of the instrument known as the Russian Transit. It was made in the workshop of the Pulkowa Observatory.

I began the observations about eight o'clock in the morning of Nov. 13, by comparing the readings of the circle with bubble first at the middle of the tube and then at the extreme end. Proceeding in this way with each five divisions in succession I was surprised to find, not only a continued diminution of the value of one division, but a well defined shifting of the zero of the level. By noon I had nearly completed the examination for the first half of the divisions. I then opened the shutters for an observation of the sun. After an interval of ten minutes, observations with the level were resumed when it was found that the value of one division, determined from the same space as before, had increased by one-fourth of its mean value. It will be sufficient to give in illustration the results of the observations on three days.

	1881 Nov. 13.	Nov. 14.	Nov. 15.
Shutters closed.	1 div = 1.76″ Th. = 38°	1 div = 1.20″ Th. = 40°	1 div = 2.06″ Th = 47°
Shutters open.	1 div = 2.08 Th. = 65°	1 div = 2.06″ Th. = 70°	1 div = 1.78″ Th = 36°

In these observations the level was held in position in the Y's with a light spring. A second series of observations was made with the tube mounted as follows: At the suggestion of Mr. George B. Clark, oval rings of brass were fitted loosely upon the tube and held in position with wax. These rings were then placed in

Y's one end being fastened with a spring clip while the other end was free. A third series was made with the tube fastened directly to the cube of the telescope at its neutral points by means of a hard cement. The results are given below.

SERIES I.		SERIES II.		SERIES III.	
1881 May 22	1 div = 2.90"	1881 Nov. 23	1 div = 1.63"	1881 Oct. 7	1 div = 2.25"
" 22	= 2.68			" 7	= 2.25
Sept. 1	= 2.60	" 25	= 2.48	" 8	= 1.64
" 7	= 1.83			" 8	= 1.62
" 7	= 1.92	" 29	= 2.18	" 8	= 1.65
" 8	= 2.83			" 9	= 1.62
" 9	= 2.74	1882 Jan. 8	= 1.80	" 9	= 1.59
" 12	= 3.42			" 12	= 1.71
" 13	= 2.89	" 12	= 1.94	" 13	= 0.83
				" 13	= 1.28
				" 13	= 1.17
				" 20	= 1.23
Mean	2.65	Mean	2.01	Mean	1.57

It is hardly necessary to say that this level has been discarded as worthless. On the other hand the mounted level furnished with the Russian Transit proves to be an excellent one.

A similar test with a level invented by Mr. John Clark has given good results. The level tube is supported upon centres which are attached to a plate which revolves freely upon another plate described by the inventor as a "reference plate." This "reference plate" is attached to the cube of the telescope by three adjusting screws. The following are the steps of an observation.

The telescope pointing north, the bubble is read for position east and position west. The telescope pointing south, the level is now on the under side of the cube. Revolving the tube upon its centres 180°, the bubble is read as before. This level therefore gives not only the inclination of the axis, but the ellipticity of the pivots.

This form of level is especially adapted to the easy determination of the value of one division. I give below the results of the determinations thus far made. The separate results are the re-

sults of observations on different days. They therefore involve the accidental errors of observation.

												Means.
1881.	Sept.	2.78"	2.71"	2.79"	2.80"	2.96"	2.87"	2.87"	3.07"	2.63"		2.83"
	Oct.	2.61	2.90	2.79	2.53							2.73
	Nov.	2.82	2.88	3.11	3.46	2.85	2.84	2.91	2.95	2.69	2.71	2.98	2.74
	Dec.	2.85	2.96	3.13									2.98
1882.	Feb.	3.21	2.97	2.69	2.77	2.99	2.62						2.87
	Mar.	2.73	3.21										2.97
	Apr.	2.92	2.80	3.01									2.91
	May	2.85	2.93										2.89
	June	2.87	3.01										2.94

The steadiness of the level of the instrument is something remarkable as is shown by the following separate observations which are here published by the permission of Professor Pickering the Director of the Observatory.

1882 Feb.	3	$b = +.78^s$	Apr.	3	$b = +.77^s$	June	25	$b = +.74^s$
"	5	$b = +.81$	"	14	$b = +.80$	July	3	$b = +.63$
"	14	$b = +.82$	"	24	$b = +.81$	"	10	$b = +.72$
"	23	$b = +.80$	May	1	$b = +.79$	"	31	$b = +.61$
"	26	$b = +.80$	"	15	$b = +.75$	Aug.	16	$b = +.71$
Mar.	5	$b = +.78$	"	29	$b = +.72$	"	25	$b = +.68$
"	14	$b = +.73$	June	11	$b = +.61$	Sept.	7	$b = +.66$
"	20	$b = +.74$	"	19	$b = +.66$	"	25	$b = +.61$
						Oct.	2	$b = +.65$

On the Arithmetic of Chords. By C. P. Hart, of Wyoming, Ohio.

[ABSTRACT.]

Ancient trigonometry, which gave birth to what is known as the "Arithmetic of Signs or Chords," though long superseded by the much more comprehensive and convenient processes and formulæ derived from the circular functions, developed by modern trigonometry, is nevertheless of considerable practical value in some trigonometrical calculations, especially when the usual logarithmic tables do not happen to be at hand. Indeed, in the solution of

certain cases of plane oblique-angled triangles, the arithmetical solution by chords is as simple and expeditious as is that of plane right-angled triangles by the Pythagorian problem, from which the arithmetical relations were originally derived.

The author, in developing these relations, discussed the following formulæ, beginning with the most simple, and obtained definite expressions for both acute and obtuse-angled plane triangles — first, for every fifteen degrees, and finally for all angles whatsoever, the latter being the equivalent of the well-known trigonometrical expression, $AC^2 = BC^2 \pm 2 \cos. B \times BC. BA.$

FORMULÆ:

θ 60°. $BC^2 = AB^2 + AC^2 - AB. AC.$

θ 120°. $BC^2 = AB^2 + AC^2 + AB. AC.$

θ 45°. $BC^2 = AB^2 + AC^2 - AB. AC \times \sqrt{2}.$

θ 135°. $BC^2 = AB^2 + AC^2 + AB. AC \times \sqrt{2}.$

θ 30°. $BC^2 = AB^2 + AC^2 - AB. AC \times \sqrt{3}.$

θ 150°. $BC^2 = AB^2 + AC^2 + AB. AC \times \sqrt{3}.$

θ 15°. $BC^2 = AB^2 + AC^2 - AB. AC \times (2 + 3^{\frac{1}{2}})^{\frac{1}{2}}.$

θ 165°. $BC^2 = AB^2 + AC^2 + AB. AC \times (2 + 3^{\frac{1}{2}})^{\frac{1}{2}}.$

θ 75°. $BC^2 = AB^2 + AC^2 - AB. AC \times (2 - 3^{\frac{1}{2}})^{\frac{1}{2}}.$

θ 105°. $BC^2 = AB^2 + AC^2 + AB. AC \times (2 - 3^{\frac{1}{2}})^{\frac{1}{2}}.$

GENERAL FORMULA

For all oblique-angled plane triangles: $BC^2 = AB^2 + AC^2 \pm AB. AC \times BB'$ (the chord of the *difference* between the contained angle and its supplement) of a circle whose radius is one.

CIRCULAR COÖRDINATES, AND COMPLEX ANHARMONIC RATIOS. By WM. WOOLSEY JOHNSON, of Annapolis, Maryland.

[ABSTRACT.]

THE *position ratio* of a point referred to two fixed points A and B being defined as the ratio AP : BP, AP and BP being *directed lines*, is a complex quantity which may be written

$$\rho e^{i\theta} = \rho (\cos \theta + i \sin \theta) = x + iy$$

ρ and θ, or x and y, may be taken as real coördinates of P; and

it is shown that the loci of $\rho =$ constant, $\theta =$ constant, $x =$ constant, $y =$ constant are all circles. When B is removed to infinity, we have ordinary polar and rectangular coördinates. Substituting x for $1-x$ we have a symmetrical system of xy coördinates, in which it is shown that the equation of a curve is the same as that of its inverse in ordinary rectangular coördinates.

P may also be determined by the ratio of its position ratio to that of an assumed fixed point Q not on the line A B. This is a complex anharmonic ratio; when put in the form $\rho e^{i\theta} = x + iy$ the loci of $\rho =$ constant, etc., are still all circles.

The anharmonic ratio is real for four points situated on the same circle. In particular the case of harmonic ratio is considered and the general construction of harmonic conjugates is given. If two circles cut orthogonally, harmonic conjugates on one of them with respect to the points of intersection are on the same diameter of the other, and if three circles are mutually orthogonal their intersection forms three mutually conjugate pairs of points. The same construction determines points whose anharmonic ratio is i.

INVERSE ELLIPTIC FUNCTIONS AND THE IMAGINARY PERIOD. By WM. WOOLSEY JOHNSON, of Annapolis, Md.

[ABSTRACT.]

THE elliptic functions sn u cn u dn u, with their reciprocals and ratios, constitute twelve elliptic functions which, according to a notation proposed by Mr. J. W. L. Glaisher, are denoted by sn, cn, dn, ns, nc, nd, sc, sd, cs, cd, ds, dc. These may be regarded as the ratios of four quantities s, c, d, n, of which only the ratios are dealt with in homogeneous equations. The derivatives of the functions are expressed in this notation, and from them are deduced the values of the inverse functions expressed as integrals.

From the latter the usual expressions for the direct elliptic functions of an argument of the form $i\,v$ in terms of functions to the modulus k' are readily derived, and these are used to deduce the imaginary period of the elliptic functions.

CONSERVATION OF SOLAR ENERGY. By PLINY EARLE CHASE, of Haverford College, Pa.

[ABSTRACT.]

ALL forms of solar energy are due to solar radiation. The maintenance of the energy depends on the maintenance of the radiations. In investigating the relations of centripetal and centrifugal action and reaction, it seems desirable to consider the following hypotheses and conclusions:

1. Laplace's estimate that the velocity of transmission, in gravitating acceleration, if finite, is at least 100,000,000 times as great as the velocity of light.

2. Le Sage's hypothesis that gravitation and luminous radiation represent equal actions and reactions.

3. Faraday's search for a gravitating constant.

4. Herschel's comparison of the mean *vis viva* of light with that of sound.

5. Weber's identification of the velocity of light (v_λ), with the "electromagnetic ratio (v_e).

6. Berthelot's "explosive waves" and their action on waves of sound.

7. Siemens's inquiries into the combined influence of rotation, centrifugal action, gravitating fall, and chemical affinity.

To these considerations the following may be added:

8. If there is a natural unit of force, we may look for a natural unit of velocity.

9. Oscillations may be orbital, pendulous, or wave.

10. Different transformations of similar oscillations are harmonic.

11. Rotation may be regarded as a pendulous motion due to retarded and modified revolution.

12. The resemblance of Le Sage's theory to the kinetic theory of gases points to a probability that the natural unit of velocity is oscillatory. This probability is strengthened if we assume the existence of molecular and intermolecular elasticity.

13. In looking to the activities of the principal mass in our system, for indications of a natural unit of velocity, we find that gravitating velocities may be represented by gt.

14. In order that gt may be constant, t must vary inversely as g and, therefore, directly as r^2. This variation is found in the rota-

tion of a nebulous sphere, where it holds good for all stages of expansion or contraction which are not affected by external influence.

15. Gravitating acceleration should do its whole work in stellar rotation, as well as in planetary revolution.

16. Particles exposed to solar superficial gravitating acceleration during a single oscillation of half-rotation, would acquire a constant velocity which is equivalent to the velocity of light. If we designate this velocity by v_γ we have $v_\gamma = gt = v_\lambda$ as a gravitating constant, which gives the following extension to Weber's analogy; $v_\lambda = v_e = v_\gamma$. In other words, the unit of velocity which is indicated by solar rotation, is the same as is indicated by light and by electricity.

17. The velocity of light, like the velocity of sound, thus represents an elastic atmosphere whose height, if homogeneous, would be twice the virtual fall which would give the velocity in question, and whose elasticity is in harmonic accordance with solar rotation and planetary revolution.

18. Subsidence, from Laplace's limit of synchronous rotation and revolution to the poles, gives a mechanical equivalent of 76,000,000 J for each pound of subsiding matter. The spiral character of the subsidence produces solenoidal currents which may help to explain the equality of v_λ, v_e and v_γ.

Correction of a Problem in "The System of the World" by Sir Isaac Newton. By De Volson Wood, of Hoboken, N. J.

[ABSTRACT.]

It is stated "that two spheres of the same material as the earth, each one foot in diameter, placed in void space, $\frac{1}{4}$ of an inch from each other, would not come together by the force of their mutual attraction in less than a month's time."

I find that they would come together in 385 seconds, or in less than $6\frac{1}{2}$ minutes.[1]

[1] Some eminent scholars are of the opinion that the above named work was not written by Sir Isaac Newton, although his name is on the title page.

On an Experimental Solution of a Problem in the Doctrine of Chances. By T. C. Mendenhall, of Columbus, Ohio.

[ABSTRACT.]

At the meeting of the Association in 1879, the author had communicated a note upon a problem in chances attributed to Buffon, which contained the results of 12,000 trials. The problem is: If a thin stick of given length be thrown at random upon a plane surface covered with a system of equidistant parallel lines, what is the probability that it will lie across one of them? In the Proceedings of the Association for that year, a description of the apparatus and mode of making the experiment will be found.

This series of 12,000 trials was made by different individuals, and the author had resolved to carry out a more extended series alone in order to avoid a "mixing" of personal equations, if such should be found to exist. The number of trials made was 20,000. No reductions were made until the entire series was completed. At the meeting of the Association in 1881, the author had communicated the results of a reduction of about half of the total number of experiments and the results of the complete reduction were now placed before the Section. The theoretical probability involved as one of its factors π, the ratio of the circumference to the diameter of a circle. It was therefore possible to obtain a value of this constant from an experimental solution of the problem. The results of the reduction were, for convenience, put in this form showing the value of π at various stages of the experiment.

Reductions were made at the end of every one hundred throws thus giving two hundred values of this constant.

A chart was shown upon which these values were plotted together with a straight line drawn to show the true value.

The experimental curve for a time fluctuated above and below this line crossing it nine times. At the end of 7200 throws the experimental value approximates very closely to the true value. After 7900 it rises above the true value to which it does not again return. From this point until the end it gradually departs, with more or less irregularity, from the true value, and with a tendency apparently towards a constant value somewhat too great.

The final value of π is 3.180, a point which is almost constantly held during the last two thousand trials. This result seems to indicate something of the nature of a "personal equation" or bias. The only explanation for it seems to exist in the decision of doubtful cases. All cases were decided, no doubtful ones being recorded as such. The deviation from the theoretical law might be accounted for by supposing that in settling the doubtful cases there was an unconscious tendency to favor one side.

Bell Attachment for Telescope Circles. By Henry M. Parkhurst, of New York, N. Y.

[ABSTRACT.]

The bell is attached to the circles, and so arranged as to indicate when the telescope passes a designated right ascension or declination. The plan for a right ascension circle is described in the Science Observer. I have now constructed one for the declination circle, which I have here, together with photographs of the whole.

The main point is the mode of avoiding the uncertainty of position of the striking. A compound lever is so arranged that whichever way the telescope is moved, the acting portion of the lever will stand at an angle of 45°, resting upon and drawn over an arc, falling into depressions in that arc, causing the bell to sound when passing the centre of the depression, and consequently always at the same position.

The method proposed allows two strokes to come within 1° of each other, so that in sweeping in either direction the stroke will come exactly at the right point.

THE TWENTY-THREE INCH TELESCOPE OF THE HALSTED OBSERVATORY AT PRINCETON. By C. A. YOUNG, of Princeton, N. J.

THIS instrument, which has been erected in the Halsted Observatory at Princeton within the last few months, was made by Alvan Clark & Sons, of Cambridgeport, Mass. The contract, bearing date March 3, 1880, and providing that the telescope should be completed within two years after the receipt of the disks of glass from the makers, has been carried out fully and promptly in all respects.

The disks were cast by Feil of Paris; one of them (the flint) was on exhibition at the Philadelphia centennial.

The material is unusually excellent, almost perfectly free from air-bubbles and striæ, and much more transparent than the disks (by Chance) used in the construction of the object glasses of the Washington and McCormick telescopes.

The clear aperture of the lens is twenty-three inches, and the curves are approximately as follows, according to data furnished by the makers:

Crown,	radius of outside surface	+ 265.8	inches
"	" inner	— 81.9	"
Flint	" " surface next crown	— 73.4	"
"	" " " focus	+ 222.2	"

The + sign denotes that the centre of curvature is on the side of the lens next the eye, the crown being a double convex lens, with the external surface nearly flat, while the flint is a double concave, with the flatter surface next the eye.

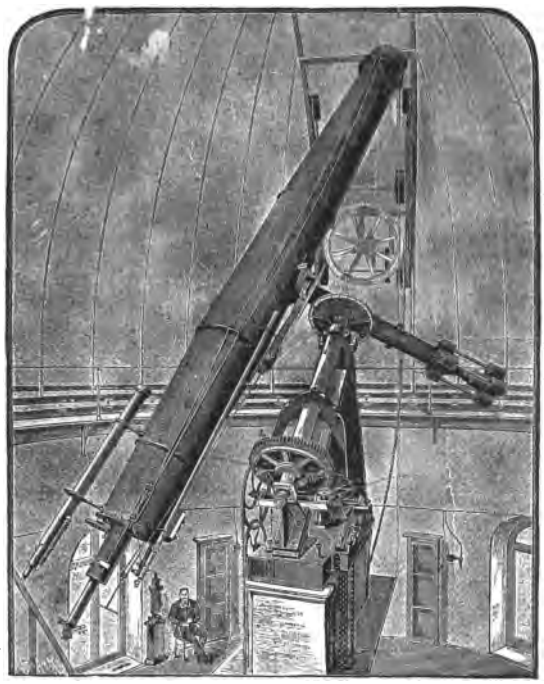
The thickness of the crown lens is	1.35	inches
" " flint "	0.62	"
The distance between the lenses is	7.61.	

The effective focus of the lens, obtained by observing star transits over a scale ruled on glass, has been found to be 361.26 inches. This is nearly a foot greater than the distance from the outside surface of the object glass to the focus, showing that the *nodepunkt* is some distance in front of the lens and not between the two glasses.

The definition is admirable, the corrections both for spherical and chromatic aberration being excellent. The "color-curve" ob-

tained by spectroscopic observation is substantially the same as that of Fraunhofer's glasses:— not quite so good as that of the Gauss object glass. The extreme range of focus between H and E amounts to just two inches.

The separation of the lenses effectually prevents the formation of any "ghosts" by reflection from the surfaces, and as the space between the lenses can easily be thrown open to the external air



THE PRINCETON TELESCOPE.

by removing the shutters which close the openings in the cell, the glass assumes the temperature of the external air very promptly. Another advantage gained is in making the aperture of the lens half an inch larger than it otherwise could be with the flint disk of given size.

There has not yet been time to test the telescope very thoroughly by actual work, but on one or two nights of good seeing, such double stars as β Delphini, and ζ Sagittæ have been satisfactorily resolved;

and Mimas, the smallest of Saturn's satellites, has been seen not only at elongation, but very near conjunction also.

The mounting presents very few new features. It is simple and solid, considerably heavier, and better planned with respect to vibrations, than that of the Washington telescope.

The tube is cigar-shaped, of steel about $\frac{1}{16}$ of an inch thick.

At the lower end a heavy brass tube, nearly six feet long and seven inches in diameter, projects about two feet, and can be moved in and out over a range of two feet by a steel screw an inch in diameter which is concealed in the lower section of the steel tube between it and the brass tube. The screw acts upon a nut attached to the brass tube, and one revolution of the hand wheel, which turns the screw, slides the brass tube $\frac{1}{10}$ of an inch. At the end of the brass tube the tail-piece proper screws on, for carrying the micrometer, or eyepieces.

The diameter of the steel tube where it is attached to the declination axis is thirty inches. From the declination axis to the object-glass extremity is sixteen feet six inches: to the lower extremity of the steel tube, ten feet nine inches, and here the diameter is twenty inches.

The total length of the right ascension axis is about nine and one-half feet. Its diameter at the lower bearing is six inches; at the upper, eight inches where it rests on friction rollers. There are two hour-circles. One, at the upper end of the axis, thirty inches in diameter, carries a coarse graduation both on the face and edge. There are three pointers, so that by means of one or other of the three, the circle can be read from any point in the room.

The fine circle, twenty-eight inches in diameter, is at the lower end of the axis, and is read to single seconds of time by two opposite verniers, which are easily accessible from a platform upon the pier.

The declination axis is nine feet long; seven inches in diameter at the bearing nearest the telescope, and five and one-half at the other bearing. The declination circle, thirty inches in diameter, is at the end of the axis next the telescope. It carries a fine graduation (to 10′ of arc) on the face next the telescope, and a coarse graduation on the edge. The coarse reading is by means of a simple pointer, and is easily made from the floor, to within 10′,— quite accurately enough to bring any object into the finder.

The fine graduation is read by means of two long microscopes

which come down to the eye end. The telescope tube carries two indices, each consisting simply of a fine line like the zero line of a vernier, and the micrometer at the eye end of the microscope gives the means of measuring the distance between this zero line and the nearest graduation mark; so that the accuracy of the reading is independent of the stability of the long microscopes. The reading is directly to 10″, and by estimation to single seconds.

The field of view of the microscope covers about 2½° of the graduation, and each degree is numbered. The illumination of the scale is on the principle of the collimating eyepiece, and is so excellent that the reading is very easy and accurate.

The clamps and tangent screws of both the right ascension and declination movements are managed from the eye end: the former by endless cords, the latter by Hooke's joint-handles.

There is an excellent apparatus for rapid motion in right ascension, by which the instrument can be reversed in less than half a minute.

The clockwork is very powerful, driven by a weight of about three hundred pounds with a fall of twelve feet in two hours. The regulator is on the same principle as that used by the Clarks on their chronographs, differing only in this respect that it is made very heavy (weighing over twenty pounds) and the weight is taken off the lower pivot of the vertical shaft by floating it in mercury. It runs very steadily, but seems to be somewhat affected by changes in temperature.

The instrument has two finders, one of five, the other of two inches diameter.

The tail piece of the telescope and of the five inch finder are both of the same size as that of the tail piece of the 9½ inch equatorial of the School of Science Observatory, so that all the eyepieces and minor apparatus belonging to either instrument can be used on any one of the three.

The sliding tubes of all the tail pieces are graduated, a great convenience in setting spectroscopic and other apparatus to focus.

The instrument is provided with a filar position micrometer, fitted with Burnham's illuminating apparatus.

It has also a double image micrometer, and a square-bar micrometer for comet observations. There is a large helioscope with polarizing reflectors, and an ordinary Herschel solar-eyepiece for the finder.

A very large and powerful direct-vision spectroscope by Hilger of London has been provided for stellar work.

It is upon the same plan as the Greenwich spectroscope (except that it is a little larger), containing three so-called "half prisms," and was constructed under the kind personal superintendence of Mr. Christie, the present Astronomer Royal, who first devised this peculiar form of prism. The spectroscope collimator is 2½ inches aperture and 38 inches long. The view-telescope has the same aperture, but its focal length is much less. The instrument is over six feet long, and weighs about 150 lbs. with its appendages.

The telescope with its mounting weighs a little more than seven tons. It stands upon the top of a heavy stone pier of Quincy granite, the upper surface of which is 14 feet above the floor. The centre of motion of the telescope (at the intersection of the Right Ascension and Declination axes) is 20 feet, 6 inches above the floor.

The dome under which it is erected is of iron, 39 feet in internal diameter. It was built some fifteen years ago, and is not just what would be constructed now for the same purpose, requiring a good deal more power to turn it around, and to manage the shutter, than is the case with some built more recently:—the Vienna dome, for instance. All difficulties, however, have been overcome by placing a four-horse power gas-engine in the basement and making it do the work by means of proper gearing. It is possible to raise the shutter, turn the dome to any part of the sky, and have the telescope pointed upon any designated object within five minutes after entering the building — this includes the time required for starting the engine. The same engine drives a small Edison dynamo-machine, which supplies electric light to all parts of the building, and furnishes electric currents for spectroscopic work.

The cost of the telescope and spectroscope was $26,000, subscribed by friends of the College of New Jersey; Robert Bonner, of N. Y., R. L. Stuart, and the trustees of the John C. Green estate being the largest contributors.

On the Law of Distribution for certain Plant-Numbers. By James Edward Oliver, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

The dimensions of any part of a plant; and also the number of flowers to a spike, of rays to a head in Compositæ, etc., when variable; have, each of them, for each species, under any given circumstances, some mean value; and I investigate, in a few typical cases, the law whereby those values of the number in question which are above or below the mean value are distributed.

I find that the number of flowers to the spike follows a law of distribution that differs but little from the normal law of distribution of errors of observation: *i. e.*, any value x has a frequency nearly proportional to the antilogarithm of $-Ax^2 \pm Bx^3 - Cx^4 \pm$ etc., wherein A, C are positive and the series convergent; as if the plant-number's deviation from the mean value were a sum of many small but sensible numbers having ambiguous signs independent of one another but dependent upon accidents of environment, ancestral history, etc.

I get a similar result concerning the *logarithm* of the length of a grass-head: indicating that the ratio of the length to a mean value is a product of many factors, each factor being a little more or less than unity as accident has determined independently of the others.

In the case of one Composita (*Leucanthemum vulgare*) I find that the distribution of the ray-numbers is strongly influenced by the "phyllotactic numbers" 13, 21, 34, . . . ; and this influence presumably combines with that of the above-mentioned law in such wise that, whenever the conditions are approximately uniform, the probability of the occurrence of a given ray-number shall differ but little from the product of the probability got as above, into a function of the ray-number's position in a cycle whose zero-points are the phyllotactic numbers. My observations, however, are too few to settle this point at present. The smaller phyllotactic numbers, 3, 5, 8, are less influential than the doubles 6, 10.

I regard the investigation as only preliminary, requiring far more varied observations to complete it; but I offer it in the hope that it may set other observers at work in the same direction.

A METHOD OF FINDING THE LAW OF LINEAR ELASTICITY IN A METAL. By JAMES EDWARD OLIVER, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

ASSUMING that the different strength of cast iron (for instance) for tension and for compression should be accompanied by a corresponding difference as to elasticity, due to the occurrence of even powers in the formula

Compressive stress $= A.\,\text{strain} + B.\,\text{strain}^2 + C.\,\text{strain}^3 + \text{etc.}$,

I get the first two or three coëfficients from experiments on the deflection of a triangular bar resting alternately on its edge and on its face.

A METHOD OF ELIMINATING THE PERSONAL EQUATION IN TRANSIT OBSERVATIONS. By J. BURKITT WEBB, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

IF, during the transit, the movable wire of a micrometer could be made to move over the field at the same velocity as the star and if it could at the same time be made to bisect the latter, then a simultaneous observation of time and the position of the wire would furnish a transit observation unaffected by personal equation; we may express this otherwise by supposing the personal equation to be divided by infinity.

It would be difficult to carry this out in practice, but if, instead of dividing by infinity, we are content with a moderately large number, there would seem to be no great difficulty in thus reducing the personal equation to an inappreciable quantity.

In considering what would be necessary to effect this result there are two paths: we must either admit the usual imperfections of such apparatus and plan the mechanism in accordance therewith, or we must attempt to eliminate or control them.

An apparatus constructed in accordance with the first of these methods has already been proposed by Prof. C. A. Young, and as

it is exceedingly well contrived for giving a result free from error we will notice the main features of it. In this method the ordinary micrometer remains untouched and another horizontal slide, carrying a vertical wire, is made to move over the field by clock-work. To effect this properly a second or vertical slide is introduced (at right angles to the former) and upon it is fixed an adjustable incline, in contact with which the horizontal slide is kept by a spring. This vertical slide is driven by the clock with a velocity a certain per cent greater than that of an equatorial star, and the incline being set at an angle therewith, whose tangent equals the cosine of the declination, the vertical wire will move the same per cent faster than any other star, and will therefore creep slowly over the same. At the moment of bisection the key of the chronograph is pressed, and the current not only records the time but clamps the moving slide fast in its position, which is then determined by the ordinary micrometer. This being done the slide is released and springs up against the incline; the wire is now ahead of the star, but if the connection between the screw and the clock be made by means of a friction collar, the former can then be adjusted independently until the wire is again behind the star and ready for another observation. It is evident that this method would be affected by only the before-mentioned percentage of personal equation.

If the second method is chosen we might proceed as follows:—dispense with the ordinary micrometer and substitute therefor one of different construction. This micrometer should have a V-threaded screw as nearly perfect as possible, and it should run in two journals, one of which should be cylindrical and the other a V screw thread of zero pitch—*i. e.*, a grooved journal. The nut should be long and should embrace the screw through 90° only; it should be cut away except at its four corners, so that it would stand on the screw as by four legs. A single spring giving a thrust at right angles to the screw would now suffice to hold the nut against the screw and the latter in its journals. Such a screw would have no back-lash, and a moderate amount of wear would introduce no error in the position of the nut. The frame for holding the movable wire should be a simple extension of this nut, and would not slide in guides, the spring being so applied to the nut as to prevent its revolving about the screw axis. The screw

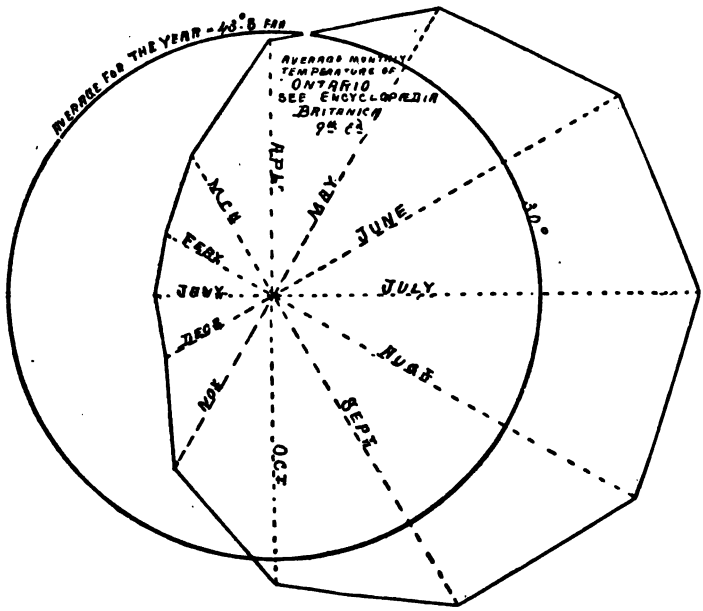
would carry the usual graduated head provided with an arrangement, by which its position could be registered (on a strip of paper) without stopping it. If this micrometer could be so accurately made that the reading could always be depended on for the position of the wire, this method could be made a success, and this reading could be at any time and often tested by means of the fixed wires of the reticule. A uniform motion could be supplied to the apparatus in various ways—it could be obtained from the chronograph by means of an interrupted current, made to drive a small electro-magnetic engine on the eye end of the transit. To reduce the motion for declination, a uniformly revolving sphere could be made to drive, by friction, a wheel so adjusted as to be in contact with a small circle of the same declination as the star,—a device which has already been proposed. A simpler method would be to cause one cylindrical surface to drive another by friction and to place the axis of the second at the declination angle with the first. With only the friction of the micrometer screw to overcome, either method might give good results. In order further to allow of single observations succeeding each other rapidly during a transit, the current which records the time and micrometer position should also check the latter until the star is again ahead of the wire, when the release of the key should start it again for another observation. A better way might be to have a separate key by which the motion could be altered from a certain per cent faster to about the same per cent slower than the star, so that the wire could be made to oscillate slowly over it. As the number of single observations during a transit would be at least equal to the usual number, an intercomparison might give a reliable value for the weight of the whole. The addition of an arrangement for reversing the motion when the telescope was reversed in its Ys would furnish the means of determining the error of the micrometer zero, *i. e.*, of collimation, and make this equally complete with the ordinary method.

NOTE ON A NEW METHOD OF GRAPHICALLY EXPRESSING ANY CYCLICAL FACT IN METEOROLOGY. By W. E. HAMILTON[1], of Chatham, Ontario.

(1) *Definition*:—By a cyclical fact, I mean, the fact of the continuous recurrence of certain quantities, in the same order, and separated by the same intervals of time.

(2) *Illustrations*:—The average lengths of sunlight for the successive months of the year give an illustration of quantities

FIG. 1.



Polar Coördinates. Invariable shape for any scale. Month = 30°.

which would reproduce themselves in the manner above described. Any monthly averages, deduced by statistical enquiry, such as the amounts of immigration in the consecutive months, for a given port, so far as they are assumed to be permanent and not accidental averages, would form a cycle of the kind referred to. The cycle which is graphically illustrated in the accompanying diagrams, is the averages of temperature for the months of the year, in the Province of Ontario, as given in the Encyclopedia Britannica (Fig. 1). So far as the observations go, on which the cycle is

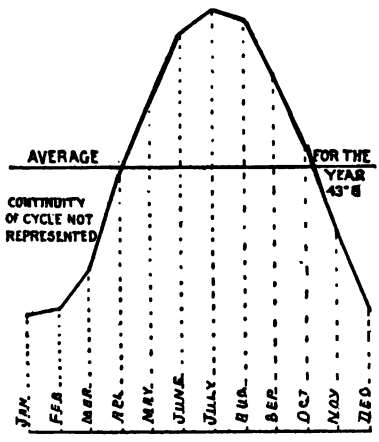
[1] Editor of the late Sir W. R. Hamilton's Elements of Quaternions.

based, it is assumed that the average monthly temperatures will recur continuously, for Ontario, from year to year, and we therefore have a cycle falling within the definition (1).

(3) *Problem:*— To express this sequence of averages graphically. It may be conceded, without argument, that no reason exists for going into three dimensions of space, and that, therefore, our diagram should be contained within a plane.

(4) *Old Solution:*—The old and usual method is to represent the element of time by an assumed arbitrary base, on which the intervals of months are represented, by arbitrary but equal spaces, separating the consecutive ordinates, proportional to the average temperatures, which are plotted on an arbitrary scale, bearing no definite relation to the arbitrary scale of months (Fig. 2). The summits of these temperature-ordinates are connected by right lines, and the unclosed rectilinear figure so constructed represents the closed and self-repeating cycle of temperatures. Temporal sequence is indicated by progression from left to right as shown by an arrow to point out the annual motion of the temperature-point. The average for the whole year is represented by a right line parallel to the base.

FIG. 2.



Ordinary method. Rectangular Coördinates. Arbitrary. Same scale as Fig. 1.

There are two objections to this method:—

(α) The visible continuity of the cycle cannot be shown, since the middle of the initial January and that of the January of the next year, are separated as the two most distant ordinates of the series.

(β) The broken line of temperatures has no invariable shape, but varies according to the ratio of the arbitrary vertical to the arbitrary horizontal scale.

(5) *New Solution:*—In my method I use polar instead of rectangular coördinates. I take a circle with an initial diameter to represent January, and successive months are separated by angles of thirty degrees from each other. On these radii, the temperatures

are plotted in their proper proportions, so that the next January temperature-radius coincides with the initial one, in length and direction. The ends of the radii are connected by right lines, and the polygon, so formed by their terminal points, represents the monthly average temperature-cycle for the year, and is visibly a cycle to the eye, while the polygon, as the simplest consideration of similar triangles will show, is of invariable shape, no matter to what scale the temperatures may be plotted. In fact, by the adoption of polar coördinates, and the consequent constancy of the angular symbol for the interval of a month, only one variable is introduced, namely, the scale of the temperature-radii. It will thus be seen that the cycle of time is patent to the eye, as a geometrical cycle of invariable shape, and that thus the objections (α) and (β) do not apply to the new method, in which, of course, the annual average is a circle with the origin of polar coördinates for centre, and a radius equal to the annual average on the assumed temperature-scale. It may be added here, that for popular illustration, the months are assumed of equal length, but that of course, in thickness, the month-angles should be proportioned to the slightly varying lengths of the months. It would be interesting did space permit, to trace the trigonometrical formulæ connecting the internal angles of the temperature-polygon, with the lengths of the temperature radii. The temperature-points might also be connected not by right lines, but by the curve of most probable curvature, which would have its evolute and other attributes of geometrical form, unchangeable in shape by alteration of the scale of temperatures. The motion of the temperature-point, in my method, is supposed to be from left to right, passing through the upper part of the figure as the spectator faces it, or in other words, the point begins to move away from the reader's body, and not toward it, when the diagram is extended before him. For monthly average temperatures in the extreme north, which may fall below zero, it is necessary, in order to avoid confusion, to take a sufficiently low temperature, say -30 for the origin of the scale of radii. In my diagrams, I assume zero of Fahrenheit, as the origin. Some minor but not utterly despicable advantages of the new method may be added. The antipode, in the annual cycle, of any given month, is pictured by a diametrically opposite point. The facts of January and July being, roughly speaking, the minimum and maximum, and the approximate equality of the fall and spring temperatures,

are patent to the scrutineer of the diagram, as is the approximate diametral opposition of the intersections of the circle of annual mean temperature with the polygon of monthly means.

(6) I submitted my method, now published for the first time, but invented more than thirteen years ago, to Prof. Kingston of Toronto, who highly approved of it, while Prof. Tait of Edinburgh thought that it was virtually anticipated by the Hodograph of my father, the late Sir W. R. Hamilton of Dublin. It is difficult in these days of general intellectual ferment to ensure priority of invention, where the energies of so many minds are converging on the same problems, and should the system have been anticipated, I can only give the right hand of greeting to my rival whoever he may be. In conclusion, I may be allowed to add that I have tried to make the explanation as full and simple as I possibly could, knowing that while a single sentence might be sufficient to give the key to the mathematical reader, there are many others deeply interested in meteorology, who would find difficulty in following a curt geometrical abstract of the system.

NOTE.—To exhibit the recurrence of the cycle, in the usual method the ordinate for January should be repeated to the right hand of the December ordinate as the representative of the mean Jan., temperature of the following year. These ordinates are replaced in my method by one and the same radius.

TITLES OF OTHER PAPERS READ IN SECTION A.

SUGGESTIONS ON THE NATURE OF COMETS, IN CONNECTION WITH RECENT ASTRONOMICAL AND ELECTRICAL DISCOVERIES. By P. H. Van der Weyde, of New York, N. Y.

SCHEME FOR OBSERVING THE GREAT ECLIPSE OF MAY, 1883. By Charles H. Rockwell, of Tarrytown, N. Y.

SECTION B.

PHYSICS.

ADDRESS

BY

T. C. MENDENHALL,

VICE PRESIDENT, SECTION B.

MEMBERS OF THE SECTION:—

NOTWITHSTANDING the many changes which have been wrought in the internal machinery of this Association by the new organization which it has put on within the past two years, and under which it now begins a sort of new life, the rule requiring an address from a Vice President has survived, and this rule will itself, I trust, render it unnecessary for me to make further apology for asking your attention briefly on this occasion.

Although this duty still lies before one who may be honored by an election to this position, yet, happily at least in the present instance, it carries with it much less responsibility under the new organization than under the old. The chairman of this section is no longer one of two, but one of nearly a half score. A Vice President is no longer expected to marshall the deeds done by half the world of Science during the past year and make them pass in orderly procession before his hearers.

We have, in fact, been shorn of even our alphabetical supremacy, an appeal to which was a last resort in friendly contests with our young and vigorous rivals of the other side. Out of respect for sciences as old as science itself we have freely accorded the first rank, as far as a designating letter may be able to imply it, to our veteran co-laborers the mathematicians and astronomers.

As re-organizers and constitution builders we may draw lines and carve out areas for which we have little respect as students of science, and no arbitrary classification can diminish the strength of the ties which bind us to Astronomy, into whose treasure-vaults we are constantly pouring rich contributions, and Mathematics, upon whose varied and wonderful resources we are obliged to draw for almost every important advance we make.

Indeed this separation of Physics from Astronomy and Mathematics as well as from Chemistry, Mechanics, etc., must be regarded as temporarily convenient for the purpose of specializing the powers of the Association.

Our sympathies, our traditions and, more than all, the constantly overlapping, intermingling currents of thought with which we are all concerned, give us the assurance that the old section, A, must continue to exist; not weaker, but stronger as a result of the digitation which has taken place, just as the fingers unite to make the hand more powerful, more cunning and more useful.

Setting out as we are upon a new and, I believe, a more efficient scheme of work, I will venture to ask the attention of this Section to a subject which may, at first, seem to many to be not quite fit and appropriate to be presented to such a body as this.

I find my justification, however, in the broad title which the general Association bears, as well as in the events of its past history. This is an Association for the Advancement of Science. The forces which have to do with aiding or retarding this advancement are so various that we are in danger of losing sight of some of them.

We are mistaken if we suppose that science is advanced only through contributions which are the result of original research in our laboratories and libraries. Even if so narrow a view be taken, it will be admitted that the talent for research is fostered and encouraged, if not, indeed, created by an atmosphere of recognition and appreciation. The existence of such an atmosphere is in itself a blessing and its production is certainly worthy of our highest efforts.

To this end it is desirable and necessary to bring about a more general diffusion of accurate knowledge concerning the elementary principles and propositions of the science of physics, as well as some degree of familiarity with the methods of physical investigation. I do not refer, of course, to the demands or the necessities

of those who expect to undergo a course of training for the purpose of becoming themselves physicists; but rather to the diffusion of this knowledge among the masses of educated people in general.

That this diffusion is not taking place to any great extent and will not, according to natural laws alone, is patent to any observing physicist, who cannot fail to have come in contact with prevailing and pernicious errors, which often carry the weight of repetition, and now and then of recognized authority.

I am aware that this is not an association of educators, and that pedagogics is not, as yet, one of the sciences specifically indicated as worthy of advancement at our hands; but if the growth of a tree is to be made healthy and permanent it is not safe to neglect the soil into which its roots penetrate. Train it and prune it as you will, to grow into vigor and strength it must spring from a rich and generous earth which, though beneath it and below it, must be in harmony with it in order to supply the proper and necessary materials for its sustenance.

It seems, therefore, not improper to raise the question, what can this Association do, or, more specifically, what can this section do to increase the efficiency of instruction in physics?

I do this the more willingly for the reason that a considerable majority of the members of the Section are engaged in this instruction during the greater portion of the year. In America a few only are privileged to devote themselves to original research. Here instruction and investigation, to a great extent, go hand in hand, and it is generally admitted that it is better so. The teacher does not reach his greatest efficiency, indeed he must fail completely unless he continues a student, not of the works of men alone but of Nature herself. On the other hand, some of the best and most fruitful inspirations of the investigator spring from his contact with those to whom he is communicating the finished products of his work. The history of science goes to show that many, perhaps most of those who have contributed to its advancement, have been great teachers. We neglect our duty, then, when we fail to give attention at proper times and under proper circumstances to the improvement of methods of instruction.

Perhaps in no other department of science has a greater change in these methods been wrought during the last ten years than in Physics. And yet this change has been going on in an irregular,

unmethodical sort of a way. There has been little or no concerted action among those interested and engaged in the work. Although all have had practically the same end in view, each individual has, in the main, worked out his own solution of the problem in accordance with his own views, modified and often largely controlled by the conditions and restrictions to which he was subjected.

It is not surprising, therefore, that results attained should in many cases be widely different and, on the whole, not entirely satisfactory.

In this new instruction many things have been attempted that could not be, and some things that ought not to be, accomplished. That over-burdened and somewhat obnoxious word *practical* has found a place in our vocabulary and we hear much of practical instruction in physics, whatever that may mean.

The subject, considered as a whole, naturally divides itself into two parts, pertaining respectively to higher instruction and elementary instruction: instruction in the colleges and instruction in the schools. Let us briefly consider each of these.

In referring to higher or collegiate instruction, it will be remembered that I do not include that of the post-graduate course in the University, properly so called, for which laboratories for research are equipped and maintained, and to which students are admitted only when thoroughly prepared by previous training. Fortunately for American students a few such courses in physics are now open in this country and it goes without saying that those who are conducting them do not need advice from us. It is for the large class of under-graduates who pursue the study of physics for a greater or less time that we may be concerned.

Contemporary with the recognition of the possibility of greatly improved methods of instruction, was the recognition of the value of the more thorough study of physics as an element in what is called a liberal education.

These were alike the results of the tremendous strides made by physical science, beginning twenty-five or thirty years ago. Grand and beautiful generalizations commanded the admiration of men skilled in other departments of human knowledge, and equally wonderful applications of principles to practice, touched our every day existence upon so many sides, as to draw forth applause from the millions who are without the "inner court." Physics thus found or forced its way upon the college curriculum to an extent

much greater than had previously been thought possible or desirable.

A few keen-sighted men, combining in themselves, happily, the student and the teacher, recognized the fact that thorough instruction in physics implied and demanded the use of laboratory methods, such as had been utilized for some years in chemistry and were rapidly coming into prominence in every other department of natural science.

Among these was, notably, Professor Pickering, whose establishment of a working, physical laboratory for purposes of instruction in the Institute of Technology at Boston, must be regarded as an epoch in the history of this progress; and with this, also, might be linked, although following at a little later date, the widely known establishment made by Professor Mayer at Hoboken.

These were quickly followed by others in the East and in the West, and at the present time there are many institutions of learning in which the laboratory methods of instruction are in use and whose equipment includes a so-called physical laboratory.

To all interested in the study of the present condition of this work I would especially recommend the very valuable Report on the Teaching of Chemistry and Physics in the United States, prepared by Professor Clarke of the University of Cincinnati, and issued about a year ago by the Bureau of Education. This Report is full of facts of great value and doubtless fairly represents the relative standing of collegiate instruction in these two important subjects at the present time.

Professor Clarke has classified the various courses of instruction in physics as follows:—

1st. Full course, including higher mathematical physics, advanced laboratory work and research.

2nd. Full course, with mathematical physics and elementary laboratory work.

3rd. Course in general physics, involving a previous knowledge of trigonometry and including laboratory work.

The other courses, up to ten in number, are elementary in their character, and do not concern the present investigation.

The report contains statistics gathered from nearly four hundred universities and colleges, agricultural colleges and scientific schools. In nearly all of these the study of physics is pursued

to a greater or less extent, although it appears that in some instances no report upon physics was forthcoming on account of ignorance as to what was meant by the word. Out of the whole number there were thirty-three institutions in which the instruction in physics fell within the limits established above. Of these there were four of the first rank, two of the second, and twenty-seven of the third.

In chemistry, however, laboratory instruction is to be found in at least one hundred and fifty institutions, the opportunities for instruction in this subject, thus outnumbering those offered for similar instruction in physics in about the ratio of five to one. But it will be remembered that in this contest chemistry has many things in its favor, and that physics is handicapped by the great cost, relatively, of the first establishment as well as by the lack of well defined and systematic courses of instruction.

Taking it as a whole it will be admitted that there has been a rapid, and, I believe, a permanent growth, and that the work has already become so extensive that it appears to be worth while to subject it to criticism and to determine by conference and consultation what improvements, if any, might be suggested.

Admitting the necessity of the laboratory as a means of instruction in physics, two important questions present themselves: first, of the total amount of time given to the subject, what proportion should be spent in the laboratory? and second, what should be the character of the work done there?

I shall not undertake to answer these questions, but will submit one or two conclusions which have been thrust upon me by observation and experience.

Concerning the first, something ought to be said. It will be remembered that the new instruction began at a time which was characterized not only by unusual scientific activity, but as well by what almost amounted to a revolution in educational processes. A great teacher had told us that we studied Nature in books, and when we met her face to face she passed unrecognized. There sprang up a new method, the essence of which was that the mysteries of nature could not be known at second-hand; that a knowledge of things could only be obtained by a contact with things themselves. The use of the text-book fell into disrepute and the student was encouraged to become his own authority. It was as

if all men were to cast aside their maps, globes, histories, books of travel, etc., and start out to obtain a knowledge of the world by visiting its different portions.

But it was soon found that many never succeeded in getting far away from home. It is true that there were those who, untrammelled by tradition, precedent or authority, made bold excursions into the regions of the unknown and returned richly laden with spoils, but these were the few; the many were found to require guidance and support for some time before they became able to carry on explorations on their own account.

The underlying principle of the new method was correct and must survive, but it was a mistake to give it universal and unrestricted application. The earlier and indeed much of the later instruction in physical laboratories was tinctured with this error.

By many we were advised that the proper course to pursue was to put into the hands of the student who, in many instances, had little or no previous knowledge of the subject, a few pieces of simple apparatus and expect him to rediscover for himself principles of physical science which although now commonplace, were at one time as completely surrounded by difficulties to the human mind as are now, for instance, the principles of the dissipation of energy and the vortex theory of atoms.

The result of the crude experiments of the student was often to disprove the law which he was expected to establish; for he lacked that knowledge and training which would enable him to take into consideration the influence of secondary causes and conditions and to determine or properly interpret the errors of experiment. Something was gained, it is true, in the way of familiarity with the methods of manipulation, but very little in the acquisition of real knowledge.

Even if this method of instruction be made reasonably successful, the actual information concerning natural laws which the student obtains must be largely superficial, often erroneous, and the rate of acquisition extremely slow. Far better would it be for him to begin his so-called practical study of the subject after becoming tolerably familiar with its general outlines and prominent features, through the study of some reliable text book and especially after having armed and equipped himself with such a training in mathematics as will enable him to discuss understandingly the results which he obtains, to consider the limitations to which they are sub-

jected and the influence which has been exerted upon them by errors of various kinds.

In this matter, as with most others, we are likely to fall into extreme views. Some of us maintain that experiment alone is the key, by the use of which Nature's mysteries are to be explored, and we fortify our belief by pointing to Faraday, the greatest experimental philosopher the world has yet produced. We forget that Faraday was ignorant only of the outward, conventional symbols of mathematical reasoning and that one of the greatest works on mathematical physics, by one of the greatest mathematical physicists of modern times is confessedly but little more than his interpretation.

Dazzled by the success of the leaders and representatives of another school, we proclaim that true success will depend on mathematical attainment, and that mathematical physics is the only physics worthy of the name. Here, again, the exceptionally brilliant few, who have succeeded under this training, stand as its exponents and we fail to consider that, if adopted to the exclusion of the first, its results may be disastrous in the extreme. No better evidence of this need be furnished than is found in the remarks recently made by Mr. G. H. Darwin, concerning a contest for honors, in what is generally admitted to be the greatest school of mathematics and mathematical Physics in existence. Mr. Darwin, who was one of the examiners, says, "The subject which exhibited the average weakness of grasp most flagrantly was thermodynamics. A great many men had read something of it, but very few really understood what they attempted to explain. Extraordinary muddle and confusion was sent up in answer to a question on the absolute scale of temperature. On another question, while the very elements of the subject were unknown to those who answered, the same men reproduced faultlessly the algebraic calculation of the thermodynamic function for a perfect gas."

Mr. Darwin also strongly recommends such a change in the style of questions as that half intelligence may be more stringently treated and men induced to read less and master more, and to gain a comprehension of physical principles.

There can be little doubt but that the experimental and mathematical study of the subject should go on together, assuming, of course, a sufficient preliminary training in pure mathematics.

What seems desirable, therefore, at least in some instances, is

less experimental work on the part of the student and more thorough and exhaustive discussion and examination of what is done.

This leads at once to the consideration of what ought to be the nature of the work done in the laboratory. The limits to which I am confined will not allow me to enter into any lengthy discussion of this important question.

I will remark, however, that in my opinion there is much done which is neither desirable nor necessary. As a rule quantitative work alone, and that the best possible under the circumstances, should occupy the time of the student. I would relegate to the lecture table of the instructor all illustrative experiments and qualitative work necessary to a good understanding of the underlying principles of the subject, which every student should possess when he enters the laboratory. That which he gets which is of most worth in his course in a physical laboratory is not a familiarity with the principles of the science, but a training in the methods of investigation in use among physicists, including a knowledge of the use and abuse of experiment and the necessary limits of our knowledge derived therefrom. The study which he ought to make of errors, instrumental and accidental, will be of great value to him in other fields than this.

As an illustration of the lack of this sort of training, I may be allowed to mention a lecture to which I listened recently, delivered by the chief engineer of one of the leading railroads in the country. The subject was the "Great Pyramid," and in speaking of certain measurements taken in the interior he declared the results, which were given in feet, inches, and thousandths of an inch, to be *absolutely accurate*, taking especial care to disclaim anything in the nature of an approximation.

I need hardly explain that he was declaiming against the introduction and adoption of a system of metrology which has done and will continue to do much to increase the simplicity and accuracy of all measurements.

I have said that this quantitative work should be of the best quality possible. It is better for the laboratory to contain a few instruments of real precision than a large number of inferior performance and accuracy. It is not a matter of great importance upon which particular department of physics a student shall spend his time and strength. The underlying principles of this method

of study are common to all and it is a matter of experience that when a student has successfully accomplished a tolerably exhaustive investigation of one topic, involving, it may be, but a single instrument with its accessories, he is upon his feet for the remainder of the course.

To sum up, the course of study in physics for the undergraduate collegian, which I have tried to indicate, should include a sufficient training in mathematics to enable him to apply his knowledge with ease and facility to the more common physical problems; a thorough and exacting course of text book and lecture work, in which the application of his mathematical knowledge would be made, and during which all illustrative experiments necessary to a complete understanding of the text should be exhibited by the instructor from the lecture table; and, finally, this to be supplemented by a course in the laboratory in which more attention is paid to the quality than to the quantity of work done; during which every problem is discussed, as far as possible, both mathematically and experimentally and especial attention is given to the discussion of the results of experiment and of the more elementary portions of the theory of errors.

Considering the work as thus divided into three parts, I am unable to see which is the least essential.

I desire to say a few words in regard to instruction in physics in the school, about which we are, apparently, more remotely concerned. Even greater reform is demanded in this direction than in the other. Although there are numerous American text-books, I venture the remark that none have properly combined, in their making, the experience of the class room with the critical knowledge of the scholar. We may import them from Europe, it is true, as we must, also, our more advanced text books, but, although in the main vastly superior to our own, they are still not entirely suited to the wants of American schools and American pupils. These books, in the hands of teachers who know little or nothing outside of the books they use and often falling far short of that, serve to put the instruction in elementary physics in this country in a condition over which no one can grow very enthusiastic; and this in spite of the prominent place which has been given it and the considerable attention it has received. Unsound doctrines and absurd theories are promulgated because of an inability to distinguish the ring of genuine metal. These become so deeply

rooted that it is difficult and often impossible in after years to clear them away.

I believe it to be possible for this Association to exert a strong influence in favor of an improvement in the character of the instruction in physics in both elementary and higher institutions of learning in America. Much of it at present does us no credit and must eventually do us much harm. At a meeting of this Association in Nashville, five years ago, a committee was appointed to report upon Science teaching in the Public Schools. At the Boston meeting, two years ago, this committee presented a report which embodied much thought upon the subject and was replete with just and keen criticisms of present systems. It is greatly to be regretted that this report cannot have found its way into the hands of those whom it would most benefit. A wide distribution ought to have been secured, and I am convinced that it is not yet too late to remedy this error.

The same gentlemen were continued as a committee to report upon the Best Method of Science Teaching in Public Schools, and it is to be hoped that a scheme may be presented at no distant day. I will venture the opinion, however, that the best results will not be obtained until this, or a similar, committee shall work in coöperation with representatives of the Public Schools themselves, and I would suggest the feasibility of securing such coöperation through the National Educational Association.

No such difficulty is in the way of securing an improvement in the teaching of physics in Colleges and Universities, for those most interested are, in the main, a part of this Association and of this Section.

I will not venture to suggest in what manner the Association might best make itself felt in this matter, although I think that would not be difficult to ascertain. I have only endeavored to direct attention to some of the salient features of the problem and to ask its consideration at the hands of many members of the Section who come in almost daily contact with it and who will, I am convinced, sustain me in the belief that it is not unworthy to be brought before this body.

In preparing this discussion of the subject, I have not had access to such information as would have been desirable, concerning the work which is being done in many institutions, nor have I

been able to consult with others who are especially engaged in its management. Indeed it was this very lack of accessible information, this very impossibility of personal consultation, with which I met in the outset, that convinced me of the importance of directing the attention of the Section to the subject.

What the Section *could* do in the direction indicated seems to be tolerably clear and certain. Whether it is wise or desirable that it should undertake to do anything is a matter which I willingly leave for it to determine.

PAPERS READ.

On the Apparent Size of Magnified Objects. By Wm. H. Brewer, of New Haven, Ct.

[ABSTRACT.]

An object seen magnified in a compound microscope by different persons impresses them very differently as to its apparent size. It is well known that such magnified object seems very much larger to some persons than to others, and a few data pertaining to this fact have been published.

Some years ago, when I had classes annually in microscopy, it was my custom to illustrate this difference of mental impression by having each member of the class in turn mark his estimate of the apparent size of some object, and when all had done, exhibit to them the estimates. This always provoked discussion, reëxaminations of the object, and wonderment on the part of some that others saw it so much larger or smaller than it appeared to themselves.

Inasmuch as I knew of no publication of specific experiments pertaining to this matter, I finally determined to keep a record of such estimates, and see what relation the average bore to the standard adopted by microscopists. Such record was begun with the estimates of my class in May, 1873. To this were added the estimates of succeeding classes. In 1876 I ceased giving instruction in microscopy, but these experiments have been continued upon such other persons as I have had opportunity to examine, until now I have a number of observations sufficiently large to give some results.

At one time or another I had used various objects for this experiment: a circular diatom, a ruled scale (as has since been used by other experimenters), an insect, and other objects; but for reasons to be mentioned later, I finally chose a small insect as best suited to my purposes.

The insect chosen was a head-louse (*Pediculus capitis*), mounted in glycerine jelly, an unusually fine specimen. I used a binocular stand, a 1½ inch objective, which, with the ocular used gave a magnifying power of about thirty-nine diameters. The estimates recorded were given in answer to a question stated somewhat thus: — "About how long, from snout to tail, does this insect *appear* to you as you see it magnified in the instrument?" If further explanation was asked, as it very often was, it was given, that the object of the inquiry might be clearly understood. Some of the observations were made with a 3-inch, a 6-inch, a 12-inch and a 24-inch rule lying on the table for convenient comparison; most of the other observations with a foot-rule similarly at hand.

The image projected with camera lucida upon a sheet of paper ten inches from the axis of the instrument, gives the following data: — length of image "from snout to tail," 4.66 inches (118.4 mm.); greatest dimensions including antennæ (or as some observers expressed it, "horns and all") 4.87 inches (123.7 mm.); diameter of field 5.85 inches (148.6 mm.). The insect was therefore entirely within the field with some room on all sides and yet extending far enough (about ⅘ across the field) so as not to appear as a minute object in a large circle of light. The instrument was always inclined at a suitable angle for comfortable observation, the object viewed by transmitted light, and nearly all the observations were by daylight.

The number of persons whose answers I have recorded is 443. About 230 of these were students in the Sheffield Scientific School of Yale College, many of whom were accustomed to use the microscope. Of the remainder, thirteen were children under twelve years of age, a few were ladies, over a hundred were mechanics and workmen, others were professional men, etc., some of whom were experts in the use of the instrument. A large house is in course of building near my own, and one day I took the microscope to where the workmen were taking their "nooning," and with the instrument set on a pile of bricks as a table, they took turns in viewing the object and making their estimates; at another time the workmen on a bridge were tried; other days I visited the machine shops near the college, and thus in a variety of ways the subjects for examination were caught.

The persons experimented upon thus embrace a great variety as to age, intelligence and vocation, from veterans in the use of the mi-

croscope to those who were looking into it for the first time; in age from less than five years to upward of eighty-five; in intelligence from eminent scientists to Irish hod-carriers and negro laborers.

Very many of the estimates were sharp and specific, as "4½ inches," "about 6 inches" or "just about 2 inches." Others were less definite, as "5 or 6 inches," "3½ or 4 inches. In such cases I have used the mean in my tables, so it must be understood that many of the estimates were not so sharply stated as the figures in the table would imply.

Of the 443 recorded numbers, two are discarded because the question was obviously misunderstood; another said the object conveyed no idea to him of size whatever, merely form, and he could give it no size. The estimates of the remaining 440, without giving the number for each fraction of an inch, may be tabulated as follows: —

Less than one inch	2
1 inch and less than 2	37
2 inches " " " 3	64
3 " " " " 4	88
4 " " " " 5	91
5 " " " " 6	56
6 " " " " 7	49
7 " " " " 8	14
8 " " " " 9	10
9 " " " " 10	7
10 " " " " 12	8
12 "	7
14 "	2
18 "	1
2 to 5 feet	4
Total number of persons	440

As the image projected at 10 inches, measures 4.66 inches, we may say that to all those whose estimates are 4½, 4⅝ and 4¾ inches, the object appeared at its theoretical magnitude. These number forty-one, about nine per cent. There were 241 estimates, or about fifty-five per cent below 4½ inches, and 158, or about thirty-six per cent above 4¾ inches. Eighty-two persons, or nearly nineteen per cent estimated from 4¼ to 5 inches inclusive, and it is fair to say

that all these saw the object at about its theoretical magnitude. There were 236, or nearly fifty-four per cent lower and only about half as many, 122 or less than twenty-eight per cent higher. There are sixty-five estimates of 2 inches or less and twenty-two of 10 inches or more; there are ten of an inch or less and fourteen of a foot or more.

This enormous difference of mental conception, as to size, immediately suggests comparisons with the impressions different people have of the size of the moon, which is really a very different matter.

The moon is seen as a bright, round, structureless object suspended against a darker sky which spreads out on every side into a vast and practically unlimited field, and the object is at an unknown but evidently considerable distance. With the object seen in the microscope, all this is reversed. We have a dark object on a bright and limited field. We look into a small dark hole through which we see the open light, and just beyond the faintly visible circling edge which limits the field, we see the magnified object. It never seems more than a very few feet distant at most. Most persons whom I have questioned on this matter say that the object appears at about the distance they would hold a book to read or any object to be plainly seen; many say less than this. It never appears at a vast and indefinite distance from us, as the moon does; it always appears in near view, as a large insect might against a circular window near, or upon a limited white surface near us. Its apparent distance from us may be, and usually is, indefinite, but it is never great.

When we see any object by natural vision so plainly and apparently so near, we always form a reasonably correct idea of its size; it is only when the distance becomes very much greater, that the judgment becomes seriously perverted and then, the greater the distance the greater the liability to such perversion.

Further, comparing this phenomenon with that of the apparent size of the moon, that is a structureless disk at an indefinite distance in a measureless field; probably if it had legs, and head, and body, or was of irregular shape like a house or even a cloud, no one would say that it looked no larger to them than a dinner-plate, as many now do. As it is, there is nothing in itself, even if it were near, to suggest any size whatever. On the other hand, the object I chose for these estimates was an organism which

when magnified was not so very unlike creatures with which we are familiar. Many compared it to a cockroach, a turtle, a lobster, or some other familiar animal. For example: two students were discussing their respective estimates which differed widely; one said earnestly to the other, after again looking into the instrument, "How is it possible that you have marked eight inches? It looks to me scarcely larger than a big cockroach." The other answered, as he too again looked into the instrument, "it seems as large to my eyes as a lobster would lying on the table before me;" and similar comparisons I have heard a score of times.

As far as I have been able to see, this mental impression as to size is reasonably definite with each individual and usually does not materially change, unless the impression at first is far from the theoretical size, and is changed later by practice in drawing with the camera-lucida. To one person the object will always appear smaller than to another. For example: a graduate student in my class estimated the length 2 inches; seven years later (during which interval he had been engaged in scientific work, and had made considerable use of the instrument) I tested him again, and his estimate was 1½ inches, he having forgotten entirely that he had ever estimated it before. Another, a professor in college who constantly uses a microscope in his professional work (to get the form of objects, but who has no occasion to draw them), estimated 9 inches; two or three years later I tested him again; he answered, "6 or 8 inches." Again, after several more years had passed, as he happened to be in my room when the instrument was on the table, I tried him again as if it was merely a casual question; he answered, "about 7 inches;" the interval between the first and last observation was more than eight years. I have kept records of duplicate estimates made by more than thirty persons, the intervals ranging from a few months to nine years, and as a rule, those who see the object as of large size first, always see it so; this is true even of children. I experimented with one of my own children, a boy in his sixth year, by having him point out on a stick how long the object appeared to him, and the length he indicated was 6 inches. Six months later I tested him again, using a very different stick and he indicated 6½ inches.

Microscopists as a class do not estimate any nearer the theoretical standard than persons unaccustomed to the use of the instrument. I have run over the list and taken the estimates of

the twenty-nine persons whom I know to be microscopists of some considerable skill, and twenty-one of them, or three-fourths of their number, estimated below the theoretical size; seventeen of their estimates were less than 3 inches and six less than 2 inches. The college professor showed me the first good compound microscope I ever saw now more than thirty-three years ago, and who has used the instrument more or less ever since, estimated the length at but 1¼ inch.

While the majority of microscopists underestimate the length, mechanics and artisans are often overestimate. They usually mentally divide the object into parts and from their sum make the total, and I think that their estimates average nearer the theoretical value than would those of skilled microscopists not accustomed to draw with a camera-lucida.

Regarding the few persons who estimated several feet, one was a grocer who said that the moon always seemed very much larger to him than it did to most people,—his estimate was "2 to 2½ feet. The highest estimate was by a mechanic who was accustomed to draw and plot his work. His estimate was 5 feet. The instrument stood upon a high desk in his shop, inclined highly, he examined the object leisurely and was entirely ignorant of the estimates of any one else. Upon questioning, he compared it to a picture thrown upon a screen by a stereopticon.

With most persons, if the estimate is made quickly, the impression as to size is reasonably definite, but if one stops and tries to reason upon it, it grows less so. We can illustrate this to ourselves by looking into the instrument with one eye and at a scale with the other. If the scale is fixed, the object seems of a definite length upon it, but if the scale is retreated from or approached to the eye the object covers more or less space and the impression of definite size is soon lost. One reason for choosing the object I did for this investigation was that the interest in the species, with most observers, drew attention away from conscious calculation.

Occasionally a person is found who can picture it of any size. A professor of physics, accustomed to optical instruments, says "I can see it at will of any size from three inches to three feet, but it appears best at about six inches." And I have credited him with that estimate in my list.

I do not purpose to enter here the disputed fields of psychology into which the study of visual perception naturally leads us, but

there are many instructive things brought out by these experiments. To me the most curious has been the antagonism between the impression derived from the visual angle of the magnified object, and its apparent distance from the observer. We commonly say that the impression of size is determined by the two factors, visual angle, and the distance of the object. Indeed in these estimates it has been curious to see in how many cases the impression as to distance and size has been antagonistic. Thus a person, who had estimated the apparent size at 1¼ inches, was asked how far off from the eye it seemed, he held an object up before his eye to indicate how far off the magnified object appeared. At the distance indicated and with the visual angle actually subtended, the object should have appeared about a foot long. I have noticed many cases illustrating the same class of phenomena, where the estimates of apparent size and apparent distance were made independently of each other and without calculation, and when if one was correct, the other should have been many (even ten or more) times as large or small.

All this applies only to the mental impression of size when objects are magnified in the compound microscope. With the simple microscope there are no such great differences of impression as to magnitude. Of various other phenomena connected with these experiments, it is not the purpose of this paper to speak.

The Spectroscopic Rain-band. By Winslow Upton, of Washington, D. C.

[ABSTRACT.]

Considerable attention has been bestowed upon the variable lines in the spectrum due to atmospheric causes, especially to those due to the presence of aqueous vapor, which have been studied by Janssen, Secchi, Smyth and others. These vapor lines vary with the amount of aqueous vapor and hence can be taken as furnishing a measure of the amount present at different times. The lines near D are especially suited for this purpose and can be observed with a pocket spectroscope directed to different portions of the sky.

[The changes in the lines themselves which cause the variability of the D rain-band illustrated by chart.]

Regular observations have been made by myself with a Browning pocket spectroscope, the results of which are illustrated by a diagram giving the rain-band curve and also the amount of rainfall noted and the psychometric curve obtained from dry and wet-bulb readings.

An improvement of the instrument is suggested by introducing a shaded band for comparison.

The results obtained may be summarized:

1. The rain-band is of value in estimating the amount of aqueous vapor present in the atmosphere.

2. Its indications follow in general those of the psychrometer but have the advantage of showing the vapor present in a large portion of the atmosphere, which advantage is occasionally of use.

3. It is of some use in foretelling rain, but must be used as any hygrometer and not as an infallible indicator independent of other conditions.

4. Improvements in the instrument used are needed to increase its efficiency, before it can be regarded as a meteorological instrument.

On the Duration of Color-Impressions upon the Retina. By E. L. Nichols, of Richmond, Ky.

[ABSTRACT.]

By a modification of the method of Plateau the author has determined for six different portions of the spectrum, the rate of revolution which must be imparted to a disk, with several narrow open sectors, in order to produce an unbroken visual image of the spectral region viewed through the disk.

The rate of revolution, which may be taken as a measure of the persistence of vision, was found to be a function of the wave length and of the intensity of the ray. The results of the experiments described may be represented by a series of curves, with wave-lengths as abscissæ and duration of the impression upon the retina as ordinates. These curves show a certain resemblance to the curves for the relative luminosity of the different spectral regions,

as measured by Frauenhofer, Vierordt and Rood. The maximum is in the same position — between D and E but nearer D than E. It seems highly probable that the duration of impression is inversely proportional to the luminosity. The curves for the duration of the visual image exhibit, it is true, relatively larger values for the more refrangible rays, but these curves correspond of necessity to a very faint spectrum. The change which the curve for luminosity would undergo, were the intensity of the ray decreased, would, owing to the greater activity of the violet nerves of the eye at low intensities, cause that curve to approach much more nearly, if it did not coincide with the curves for duration of impression.

It was also found that the curve differed greatly in the case of different observers, and to a less degree from time to time, for the same eye.

The interval of darkness which may be allowed to intervene between exposures of a given duration, without interfering with the apparent continuity of vision, and its variation when the length of exposure varied, were also determined. The exposure varied from $\frac{12}{1000}$ second, to $\frac{758}{1000}$ second, and the interval of darkness was found to vary, but in less degree.

ON A MEAN-DIRECTION INTEGRATING ANEMOMETER. By CHAS. K. WEAD, of Ann Arbor, Mich.

[ABSTRACT.]

MANY forms of anemographs have been devised that record the direction of the wind at each instant as in Beckley's, or to eight points from time to time, as in the signal service record. But it is a tedious problem to obtain from these records the "mean direction" of the wind for any time by the aid of a traverse table. The instrument now described performs at each instant the resolution into an N and E component that is effected by the traverse table, and takes the sums of all similar components for any length of time.

Two equal gear wheels A and B are connected to one of the same size on the shaft of the wind-vane: on each of them a stud works in a slot in the head of the familiar yoke-headed connecting-rod; the two studs being set "at quarters," the distant end of the connecting rod will be displaced from its mean position by an amount proportional to the sine or cosine of the angle through which A and B and the vane have been turned from the meridian. On these ends friction-wheels C and D are pivoted which turn freely in a plane perpendicular to the rod. C and D slide over the faces of wheels underneath them and are driven by friction on them: these wheels, E and F, have the same velocity, and are driven either by clock-work to get a time-integration as in Coffin's tables, or by a "step by step mechanism" (as in a dial telegraph) actuated whenever a mile of wind has passed, to obtain a distance-integration. The velocity and direction in which C and D are driven will depend on their distance from the centres of E or F; that is, on the position of the studs in A and B, and so ultimately on the direction of the wind.

C and D are connected with counters or with type-wheels for printing; and the instrument is easily modified (as in the drawing exhibited), so as to find the mean direction from such old records as give the actual direction at every instant.

To find the mean direction: obviously the reading of the counter of C divided by the reading on the counter of D gives the tangent of the angle from the meridian (or the co-tangent, according to the original setting of A and B).

On Vision by the Light of the Electric Spark. By W. Le Conte Stevens, of New York, N. Y.[1]

[ABSTRACT.]

This paper is merely the continuation of an investigation, published lately in the Am. Journal of Science, in which the current theory of binocular perspective, on the principle of visual triangu-

[1] Published in full in the Am. Journal of Science for Oct., 1882; also full abstracts in the London Philosophical Magazine for Oct., 1882.

lation, has been tested in various ways. This theory still has its place in most of our text-books, being due mainly to the surviving authority of Sir David Brewster and Brücke.

The writer has shown that it fails to take into account some important physiological conditions and is wholly inadequate in many cases. He has shown that the unconscious interpretation of muscular sensation explains all that Brewster's theory explains, and much more. In studying certain mistaken observations by Brewster, he has been enabled to devise a new mode of stereoscopy, in which the perception of binocular relief, reversible at will, is secured in the examination of a pair of perfectly similar figures, by properly adjusting these in position and taking advantage of the almost spherical form of the retina in each eye. A partial explanation of this has been published, in which it was assumed that the eyes have perfect freedom of motion. Believing, however that such motion is not essential, however important it may be in completing the perception, he deemed it advisable to study this mode of stereoscopy, with visual lines parallel or divergent, by the light of the electric spark. This has been done, with the aid of Mr. W. W. Share, of Columbia College, and with apparatus loaned by Prof. O. N. Rood.

The result has been to show that the clear perception of binocular relief is attainable, not only without motion of the eyes, as first shown by Dove, but without convergence of visual lines and without the perception of double images in any part of the binocular field of view.

THE BINOCULAR UNION OF SPECTRAL IMAGES. By W. LE CONTE STEVENS, of New York, N. Y.[1]

[ABSTRACT.]

IN studying the theory of vision the writer has had occasion to test certain experiments by Professors Wheatstone, Rogers, and Wundt, on the binocular union of images obtained by fatiguing the

[1] Published in full in the Am. Journal of Science for Nov., 1882, and London Philosophical Magazine for Oct., 1882.

two retinas with the light from illuminated diagrams, so arranged as to produce the appearance of relief, and then noting whether this appearance is recognizable in the subsequent spectral images that remain visible with more or less distinctness after the actual pictures are removed.

The source of possible error in the experiments hitherto made consists in the fact that the subsequent perception is vitiated by the observer's knowledge of what he has just seen, or of what ought to be the binocular resultant under usual conditions. This objection is applicable even in cases like that detailed by Professor Wm. B. Rogers, in which the two dissimilar diagrams were presented, each to the appropriate eye, in succession rather than at the same time. This method has been adopted in the present investigation, but with the additional important precaution, that the person tested shall not know what is the nature of the relief that should result if the two diagrams were simultaneously viewed. This was then ascertained, under varied conditions *by means of* the spectral images.

The result is a full confirmation and an extension of the conclusion reached by Professor Rogers.

These experiments, like those with the electric spark, show that muscular action is by no means necessary to the perception of binocular relief, though often an important aid; also that the perception of double images, whether consciously or unconsciously, is not necessary. The writer believes that difference in the degree of attention given to different points, at the same moment seen by direct and by indirect vision respectively, is the only indispensable condition to the perception of relief, and that the interpretation of this is a product of individual experience and not, in any demonstrable degree, of intuition. In this he accords with Helmholtz in opposition to many others who have written on this subject.

Upon the Electrical Experiments to Determine the Location of the Bullet in the Body of the late President Garfield; and upon a successful Form of Induction Balance for the Painless Detection of Metallic Masses in the Human Body.[1]

By Alexander Graham Bell, of Washington, D. C.

The subject of my present paper recalls a time of intense excitement and painful suspense. The long, weary struggle with the untimely death-wound—the prolonged suffering borne so bravely and well by the lamented President Garfield—must still be fresh in every recollection. The whole world watched by his bedside, and hopes and fears filled every passing hour. No one could venture to predict the end so long as the position of the bullet remained unknown. The bullet might become safely encysted, but, on the other hand, recovery might depend upon its extraction. The search with knife and probe among vital and sensitive tissues could not be otherwise than painful and dangerous; and the thought naturally arose that science should be able to discover some less barbarous method of exploration.

Among other ideas[2] the thought occurred that the bullet might produce some sensible effect in modifying the field of induction of a coil brought near the body of the President, and that the locality of the bullet might thus be determined without danger to the patient and without pain; for it is well known that induction can be powerfully exerted through the human body without producing any sensation whatever.

Upon the balancing of Induction.

The influence that is exercised upon induction by metallic masses has formed the subject of numerous experiments by different investigators; and the principle of balancing the effects of induction on one portion of a circuit by equal and opposite effects produced upon another portion has been utilized in nearly all such investigations.

The earliest form of induction balance for this purpose appears to have been devised in Germany by Prof. Dove,[3] about the year

[1] A preliminary notice relating to this paper was published in the *Comptes Rendus* of the French Academy of Sciences, Oct. 24, 1881.

[2] See Appendix, note 1.

[3] Pogg. Ann. vol. liv, pp. 305–335.

1841, and a good description of it in the English language may be found in De La Rive's "Treatise on Electricity," (1853 edition, vol. I, pp. 418–433) [4].

Another and superior arrangement for the same purpose is the well-known induction balance of Prof. D. E. Hughes [5].

The Static Induction Balance of J. E. H. Gordon [6] though primarily intended for experiments upon specific inductive capacity, might also, perhaps, be employed in the same class of investigations.

My own attention was directed to the balancing of induction a number of years ago by the disturbing noises produced in the telephone by the operation of telegraphic instruments upon lines running near the telephone conductor.

The difficulty was remedied by using two conductors instead of one, and by so arranging them with reference to the disturbing wires that the currents induced in one of the telephone conductors were exactly equal and opposite to those induced in the other. In this way an induction balance was produced and a quiet circuit secured for telephonic purposes. This method was patented in England in November, 1877, and during the whole winter of 1877–8 I was engaged in London upon experiments relating to the subject.

In the course of these researches I made frequent use of flat spirals of insulated wire, like those employed by the late Professor Henry [7] in his experiments upon induction.

My method was to pass a rapidly interrupted voltaic current through one flat spiral while I examined its field of induction by means of another flat spiral connected with a telephone. The currents induced in the latter coil produced a musical tone from the telephone.

At every point in the field of induction it was found that by turning the plane of the exploring coil a position of silence could be obtained, and another of maximum sound, the two positions making a right angle with one another.

It was also noticed that when a position of silence was established a piece of metal brought within the field of induction caused the

[4] A similar apparatus was independently devised in America a number of years ago by Professor Rowland, of Johns Hopkins University. It is to be regretted that his discovery of the fact that he had been anticipated by Dove prevented Professor Rowland from completing and publishing his researches.

[5] Phil. Mag., July, 1879, vol. ii, p. 50.

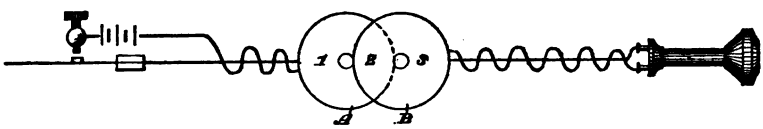
[6] Phil. Trans. for 1879, p. 417.

[7] Silliman's Journal, xxviii, 329; xxxviii, 209; xli, 117.

telephone to sound. This effect was most marked when the two flat spirals were in close proximity, and were arranged with their planes parallel, as shown in fig. 1.

When a silver coin, such as a half-crown or florin, was passed across the face of the two coils, the silence of the telephone was broken three times. The instrument emitted a musical tone when

Fig. 1.



the metallic disk passed the points marked 1, 2, and 3 in the illustration, but the loudest effect was produced when the coin crossed the area marked "2," where the two coils overlappped.

After my return to America I embodied these and other results in a paper "*Upon New Methods of Exploring the Field of Induction of Flat Spirals*," which was read before this association at the Saratoga meeting in August, 1879.

Practical Application.

While brooding over the problem of the detection of the bullet in the body of President Garfield, these experiments made in England returned vividly to my mind. It seemed to me that if the overlapping area "2" of the two coils shown in fig. 1 could be brought over the seat of the bullet without disturbing the relative positions of the coils, the telephone would probably announce the presence of the bullet by an audible sound.

A crude experiment was at once made to test the idea. A large, single-pole electro-magnet (the core of which was composed of a bundle of fine iron wires) was used in place of coil A (fig. 1;) and a small coil of fine wire taken from a hand telephone was arranged a little to one side of the pole to represent coil B. The small coil being connected with a telephone, a battery current was passed through the coil of the electro-magnet, and the battery circuit was made and broken by an assistant.

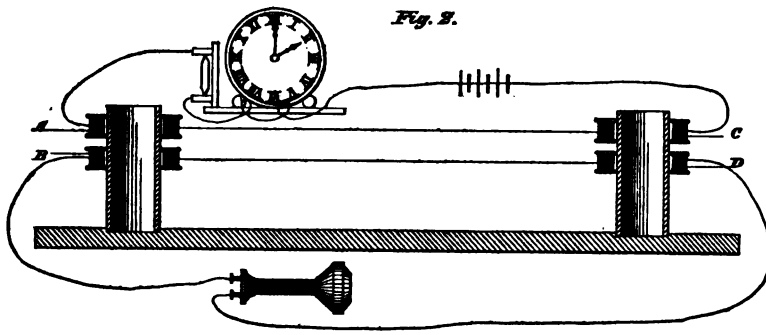
Under these circumstances a much better balance was obtained than could possibly have been anticipated. Upon now bringing a

leaden bullet near the small coil, a distinct ticking sound could be heard from the telephone each time the battery circuit was made and broken.

Being absent from my laboratory, and without facilities for proper experiment, I communicated my ideas to Mr. Charles Williams, jr., of Boston, manufacturer of electrical and telephonic apparatus, who kindly placed the resources of his large establishment at my service; and, at great personal inconvenience, delegated his best workmen to attend to my experiments.

Upon attempting to devise an appropriate form of apparatus for the special purpose in view, I saw that there were great practical difficulties in the way of utilizing the arrangement shown in fig. 1, and it occurred to me that the apparatus of Professor Hughes

Fig. 2.



might perhaps be employed with more advantage as the basis of my experiments. In the ordinary form of Hughes' induction balance four coils are used, as shown in fig. 2. Through the agency of a Hughes microphone the ticking of a clock is made to create an electrical disturbance in the voltaic circuit containing the two primary coils (A C) and a corresponding disturbance is produced by induction in the two secondary coils (B D) connected with the telephone. If the connections are so arranged that the currents induced in the telephone circuit by the coils A C are in the same direction, the ticking of the clock is heard very plainly, but if they are in opposite directions no sound is perceived.

In the latter case the action of one primary coil (A) opposes that of the other (C), and an electrical balance results. If now a piece of metal is brought near one pair of coils (say A B) the bal-

ance is disturbed and the ticking of the clock is audible at the telephone. The arrangement of the coils (A, B, C, D) was the point to be studied, the microphone attachment being of no importance in the combination; for it is well known that a rheotome to break the primary circuit completely at intervals can be substituted for the microphone with advantage.

It seemed to me that two of the coils (A B) in the Hughes induction balance might be attached rigidly to a wooden handle, so as to be moved over the seat of the bullet without changing their relative positions, and that all the adjustments necessary might be made on the other pair of coils, which need not be moved from their place, and would not therefore be liable to disarrangement. If a single pair of coils were to be used as in fig. 1, they must be adjustable one upon the other. But if during the course of exploration the coil B (fig. 1) should be moved from its proper position even to the extent only of a small fraction of a millimetre, the balance would be disturbed and the exploration might have to be stopped in order to adjust the apparatus. These considerations led me to the conclusion that some modification of the Hughes induction balance was most suitable for my purpose, and I immediately commenced the construction of such an apparatus.

Suggestions Tested.

Just at this time I learned from the newspapers that Prof. Simon Newcomb, of Washington, had the idea of using a magnetic needle to indicate by retardation of its rotation the proximity of the bullet in the body of the President, and I telegraphed to Professor Newcomb the offer of my assistance in carrying on experiments, knowing the comparative difficulty he would experience in having apparatus made in Washington.

At his suggestion I tested the point whether the rotation of a leaden disk and of a leaden bullet underneath a delicately suspended magnetic needle would cause a deflection of the needle.

The disk occasioned a deflection, but the bullet produced no sensible effect. I telegraphed the result to Professor Newcomb, and at the same time took occasion to inform him of the hopeful results I had obtained with the crudely constructed induction balance referred to above.

I was much gratified by his immediate appreciation of the ex-

periment. He telegraphed that he thought an induction balance promised a much more hopeful solution of the problem than his own method, and encouraged me in every way to continue my experiments.

This appreciation determined me to proceed to my laboratory at Washington, where I was accompanied by Mr. Sumner Tainter, who was anxious to assist in such a cause. I learned from Professor Newcomb that Mr. Geo. M. Hopkins, of Brooklyn, had independently suggested the use of Hughes' induction balance, and had made experiments in Brooklyn, the results of which were published in the New York Tribune on the 11th of July, 1881.[8] Mr. J. Stanley Brown (private secretary of President Garfield) kindly handed to me the letters he had received from Mr. Hopkins,[9] and also a Hughes induction balance like that shown in fig. 2, which Mr. Hopkins had forwarded to the Executive Mansion for trial.

This apparatus was at once tested in my laboratory, with results slightly better than those I had obtained in Boston.

My Boston apparatus did not give a greater hearing distance than 3 cm., whereas with the Hopkins apparatus I could distinguish effects at a distance of 3.75 cm.

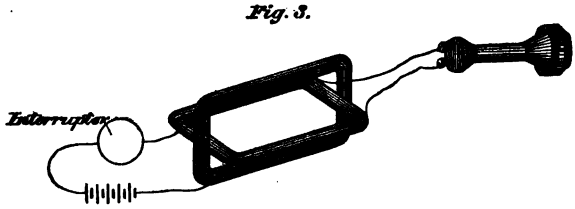
Two of Mr. Hopkins' coils (A B, fig. 2) were then fastened upon a wooden handle to form an exploring instrument, and the whole apparatus was arranged for immediate use in case of any necessity arising for an experiment upon the President. I set myself in communication with Mr. Hopkins, and requested his assistance and coöperation, and in reply received through Private Secretary Brown the following account of further experiments:

"60 IRVING PLACE, BROOKLYN, *July* 16, 1881.

MR. J. STANLEY BROWN:

DEAR SIR: I have made two new instruments on plans differing from that sent, but they yield no better results. The first consisted of two oblong coils arranged at right angles to each other, thus:

Fig. 3.



[8] See Appendix, note 2.

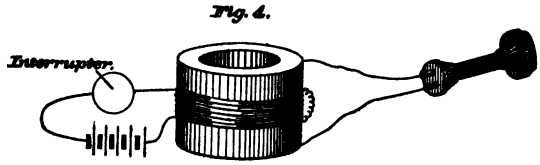
[9] See Appendix, notes 3 and 4.

The outer coil being of coarse wire (No. 18) placed in the primary circuit, the inner coil being of very fine wire (No. 36) and connected with a telephone. The parallel currents traversing the wires neutralized each other, and no audible effects are perceived in the telephone, but on presenting a metallic body to the instrument upon a line bisecting the angle between the coils the clicking in the telephone is heard.

This instrument possesses only one advantage over that sent, and that is that it requires no adjustment.

The other instrument consists of two large coils of very fine wire (No. 36) placed upon opposite sides of a coil of coarse wire (No. 16), the fine coil being connected so that the induced currents neutralize each other, thus:

Fig. 4.



I am sorry to be obliged to say of this as of the other, that it is no more sensitive than the one sent. To produce the best effects from the instrument which you have it will be necessary to use all the battery power possible without burning the coils, and two receiving telephones of the best construction must be used.

As I stated in the first instance, if the ball is more than two inches deep, I think it cannot be located by this means.

If larger coils were used the instrument might be operative at a greater distance, but the area indicated as containing the ball would be so large that the result would be indefinite and without value.

Hoping that Prof. Bell will be able to succeed, I remain,

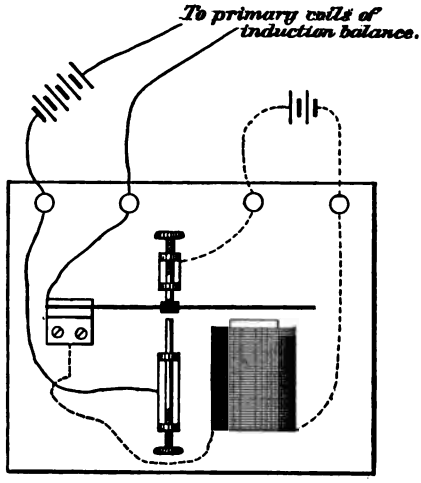
Yours very truly,

GEO. M. HOPKINS."

Prof. Hughes of London, England, Prof. Trowbridge of Harvard College, Prof. Rowland of Johns Hopkins University, and other authorities were consulted by telegraph as to the best theoretical form of induction balance for the purpose required, while empirical experiments were being carried on under my direction in my

laboratory at Washington by Mr. Sumner Tainter; in the electrical workshop of Davis and Watts, in Baltimore, by Mr. J. H. C. Watts, and in the establishment of Mr. Chas. Williams, jr., in Boston, by Mr. Thomas A. Gleason. To test the influence of size of coil an instrument was constructed in which the coils were no larger than the bullet for which we sought (as had been suggested by Prof. Newcomb),[10] and experiments were also made with the enormous coils used by the late Prof. Henry in his researches upon induction, which were kindly lent to me for the purpose by the Smithsonian Institution, but neither the small nor the large coils produced more satisfactory results than those we had already obtained.

Fig. 5.



To test battery power, twenty enormous Bunsen elements, which had formerly been used to light the gas at the Capitol, were placed at my disposal by Mr. Rogers, electrician of the Capitol, but while great electro-motive force was evidently of use we derived no advantage from such a battery as this.

To test the influence of speed of interruption, Mr. Marean, Supt. of the Western Union Telegraph Co. in Washington, kindly lent us an electric motor, by means of which we were able, with the aid of a rotating commutator, to obtain interruptions of the primary circuit of all rates up to 600 interruptions per second,[11] and we found that the more rapid the rate of interruption the more distinct was the sound in the telephone. The hearing distance, however, was not proportionately increased. The automatic interrupter (shown in fig. 5), yielding

[10] See Appendix, note 5.

[11] Mr. Sumner Tainter has since made an apparatus operating in a similar manner by means of which he has obtained as many as 4,000 interruptions of the circuit per second.

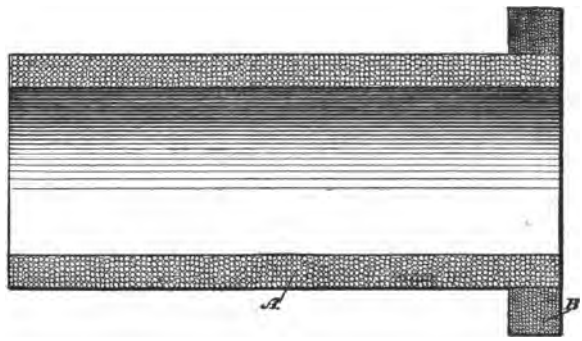
about 100 interruptions per second, gave as good results as any, and was much more convenient. This interrupter was therefore afterwards used exclusively in our experiments.

The theoretical form of coil suggested by Prof. John Trowbridge[12] was substantially the same as that proposed by Prof. Rowland,[13] and is shown in fig. 6.

The arrangement was quite sensitive to metal placed in the interior of the coil, but the hearing distance for a bullet external to the coils was no greater than before.[14]

Professor Hughes[15] proposed to have two flat superposed coils wound on a single reel, so that the two coils should form a single

Fig. 6.



one as regards their relative distance; and Mr. F. T. Bickford, Washington correspondent of the New York Tribune, suggested winding two wires side by side into a single coil, so that the relative distances of the wires from the bullet should be absolutely the same. Mr. Chas. E. Buell[16] and Dr. Chichester A. Bell[17] proposed to determine the depth of the bullet beneath the surface by causing a similar bullet to approach the balancing coils until silence was restored; the secondary bullet it was presumed would then be at the same distance from the balancing coils as the embedded bullet from the exploring coils.

[12] See Appendix, note 6. [13] See Appendix, note 7.

[14] The balance obtained was not quite perfect, and we have since discovered that the insulation of the wires of one of the secondary coils was defective.

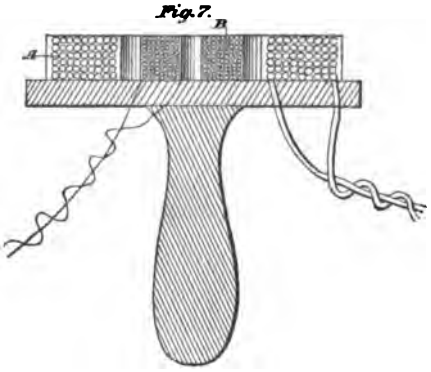
[15] See Appendix, note 8. [16] See Appendix, note 9. [17] See Appendix, note 10.

The results of all the experiments so far made were unsatisfactory. I had tried everything that had been suggested, but 4 cm. remained the extreme limit of audibility for a bullet like that which had struck the President. Even when such a bullet was flattened by being fired against a board, and was presented with its flat side towards the coils of the explorer — the most favorable mode of presentation — no better result was obtained.

Original Experiments.

In the theoretical arrangement recommended by Professors Trowbridge and Rowland (fig. 6) the primary coil A was of smaller diameter than the secondary B. This had given us no better effects than the ordinary form of Hughes' balance (see fig. 2), in which the two coils A B were of equal diameter. We then tried the effect of making the primary coil A of greater diameter than the secondary B (see fig. 7) and in this case we appeared to obtain an increase of hearing distance. Five centimetres (2 inches) was, however, the utmost limit reached, when, on the 19th of July, Mr. J. Stanley Brown and Dr. Woodward visited my laboratory and witnessed some experiments. No difficulty was experienced in detecting a bullet held in the mouth by passing the exploring coil over the cheek; and the presence of a flattened bullet held in the clenched hand was also readily determined. Dr. Bliss, Dr. Reyburn, and Surgeon-General Barnes visited the laboratory next day and expressed themselves as very hopefully impressed by the experiments. These were subsequently repeated in the surgeon's room at the Executive Mansion for the information of Dr. Frank Hamilton and Dr. Agnew, who also seemed favorably impressed.

Fig. 7.



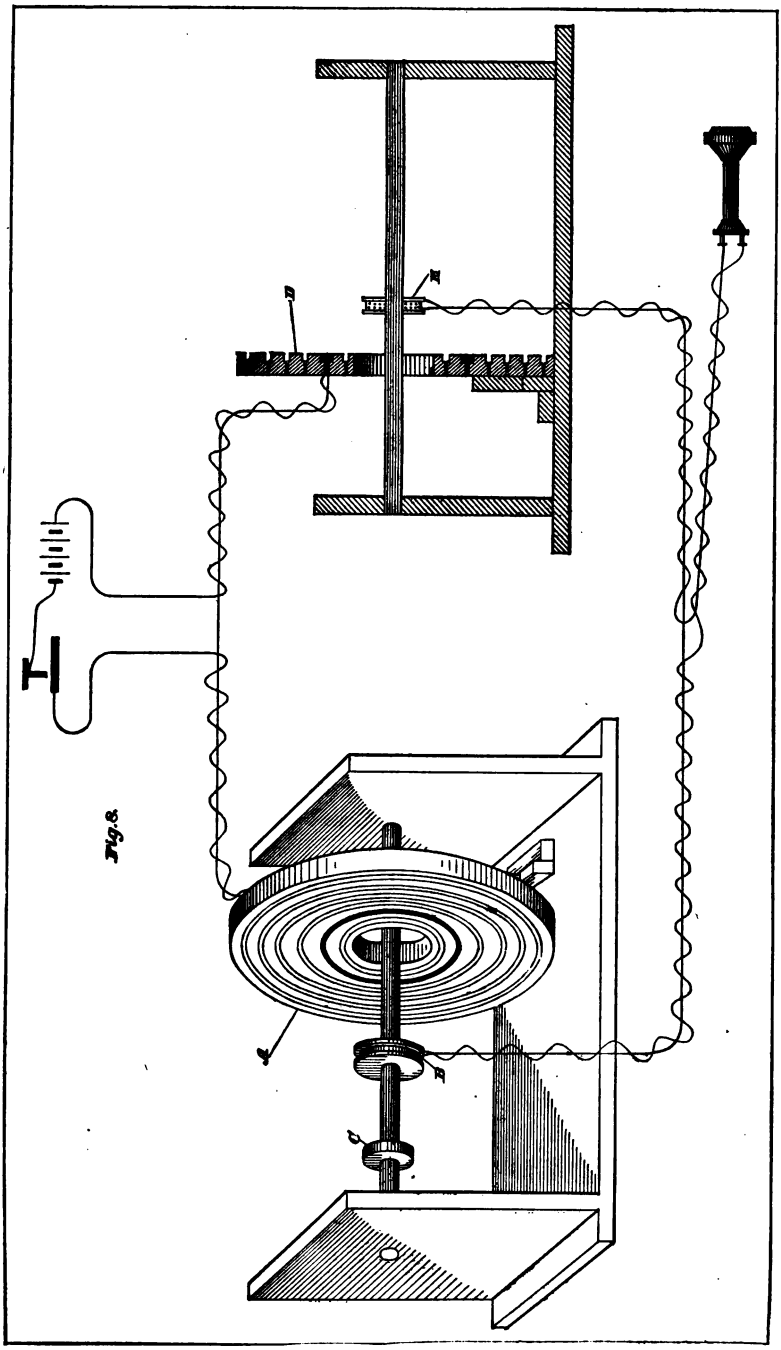


Fig. 8.

Such opinions from the surgeons in attendance upon the President, and the continued interest shown by Professor Newcomb, encouraged me to proceed with the experiments.[18]

It was now determined to test the effect of each convolution of the primary coil, so as to arrive empirically at some idea of the best shape of coil. For this purpose Mr. Tainter constructed the instruments shown in fig. 8. Circular grooves were turned in two boards, one of which is shown in perspective at A and the other in section at D. An insulated copper wire could be pressed into any of these grooves so as to give the wire an exactly circular shape of known diameter, and the two ends were passed through an orifice in the back of the board, making connection with a similar ring of wire in the other instrument as shown. A small secondary coil (B) of fine wire, which could be moved with moderate friction upon the horizontal rod, was connected to another similar coil (E) and to a telephone; and a small brass ring (C), which could also be moved along the horizontal rod, was used instead of a bullet to disturb the balance.

In making an experiment with this apparatus the secondary coil (B) was first placed within the primary ring and in the same plane with it, and the balancing coil E was adjusted to produce silence. The brass ring C was then moved along the horizontal rod until the balance was sensibly disturbed and the relative distance of the coils and the brass ring were noted.

Continuing the experiment, the coil B was moved a determined distance beyond the plane of A, and the balancing coils again adjusted to silence. The brass ring C was once more caused to disturb the balance, and the new hearing distance was noted. The following are the tabulated results of a series of experiments made on the 19th of July, 1881. The battery employed consisted of six bichromate cells connected in series.

[18] I desire especially to express my gratitude to Dr. Frank Hamilton for words of encouragement spoken at a later date when sympathy and encouragement were greatly needed.

Diameter of Primary Ring.	Distance Between—		
	A B	B C	A C
mm.	*mm.*	*mm.*	*mm.*
	0	17	17
	5	14	19
30	10	13	23
	20	9	29
	30	7	37
	50	0	50
	0	17	17
	5	19	24
50	10	26	36
	20	17	37
	30	14	44
	50	5	55
	0	21	21
	5	23	28
81	10	23	33
	20	18	38
	30	14	44
	50	12	62
	0	22	22
	5	25	30
113	10	27	37
	20	26	46
	30	26	56
	50	17	67

Diameter of Primary Ring.	Distance Between—		
	A B	B C	A C
mm.	*mm.*	*mm.*	*mm.*
	0	27	27
	5	20	25
159	10	18	28
	20	17	37
	30	14	44
	50	14	64
	0	12	12
	5	18	23
206	10	25	35
	20	19	39
	30	22	52
	50	18	68
	0	20	20
	5	18	23
253	10	20	30
	20	19	39
	30	23	53
	50	20	70

These figures show that *the distance from the primary coil* A (fig. 8), *at which the influence of the brass ring* C *became perceptible, increased with the diameter of the primary ring, and that the secondary coil* B *required to be projected considerably beyond the plane of the primary in order to obtain the maximum effect.*

The conclusion seemed a natural one that the degree of projection A B of the secondary coil should proportionally increase with the diameter of the primary ring, but the tabulated figures did not fully justify the inference.

The experiments had necessarily occupied a considerable time, and I thought that the difference between the results that should have been observed, according to the above hypothesis, and those that were actually obtained, might have been due to the gradual exhaustion of the bichromate battery employed and to its polarization, although every care had been taken to preserve its power

by removing the carbon and zinc plates from the solution, excepting when an observation was made. To test whether the battery exerted any material influence upon the hearing distance, a further series of experiments was made with the same battery.

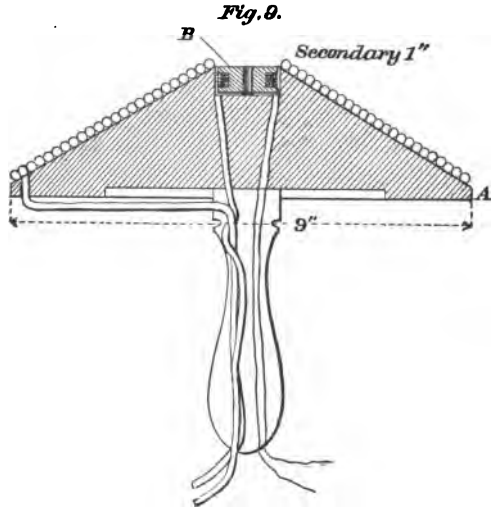
It will be seen by reference to the tabulated statement shown above that the maximum hearing distance B C had been obtained with a primary ring 11.3 cm. in diameter when the distance A B between the primary and secondary coils was one centimetre. This arrangement of the apparatus was therefore adopted throughout the following experiments:

	Hearing distance.
1. Apparatus tried with 1 cell (bichromate battery)	(B C, fig. 8) = 9 mm.
2. Six cells in series	(B C, fig. 8) = 16 mm.
3. Six cells in multiple arc	(B C, fig. 8) = 9 mm.
4. Six cells in two series of 3 each	(B C, fig. 8) = 15 mm.
5. Same experiment repeated	(B C, fig. 8) = 13.5 mm.
6. Same experiment repeated by Mr. Tainter	(B C, fig. 8) = 12.5 mm.

These experiments proved that battery power *did* exert an influence upon hearing distance, and also that the battery in use was gradually deteriorating.

I concluded, therefore, that if the battery power had remained

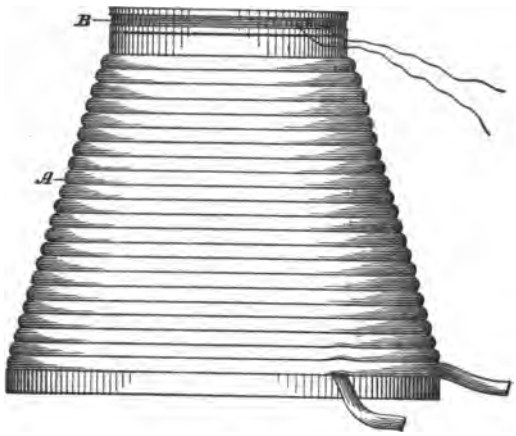
Fig. 8.



constant, the hearing distance might not only have been proportional to the diameter of the primary ring, but, in order to attain

the maximum effect, the projection of the secondary coil beyond

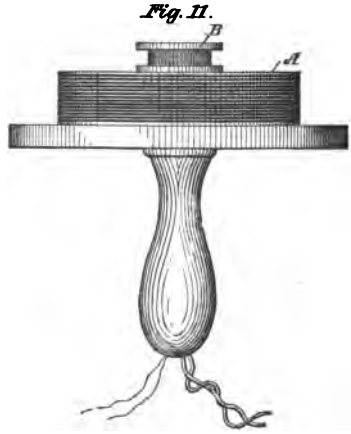
Fig. 10.



the plane of the primary might also have been found to increase in like proportion.

This led me to try the effect of a conical primary coil A with the secondary B at its apex, as shown in fig. 9, but the hearing distance for a bullet was only 3.5 cm.

Fig. 11.



Singularly enough Mr. J. H. C. Watts, in Baltimore, had independently arrived at a very similar form of coil, and with the instrument shown in fig. 10 he had obtained at one time a hearing distance of 7.5 cm. (or 3 inches[19]), but from some cause not ascertained he was unable subsequently to reproduce the effect.

The final form of apparatus adopted as the result of the above experiments is shown in fig. 11. With this arrangement and a battery of six bichromate elements freshly set up, we were always sure of a hearing distance of at least 5 cm., although after the

[19] See Appendix, note 11.

battery had been in use for some time the hearing distance hardly exceeded 4 cm.

The following are the dimensions of the coils A B (fig. 11) and their resistance:

COIL A.....External diameter . . . . . 7 cm.
Internal diameter . . . . . 4.5 cm.
Depth . . . . . 2.4 cm.
Wire used, No. 23 (cotton covered). Resistance, 2 ohms.
COIL B.....External diameter . . . . . 2.3 cm.
Internal diameter . . . . . 8 mm.
Depth . . . . . 8 mm.
Wire used, No. 36 (silk covered). Resistance, 75 ohms.

The face of the coil B projected beyond the face of coil A 4 mm.

The balancing coils were made as nearly as possible the duplicates of A and B. The resistance of the coil of the telephone employed was 75 ohms.

Influence of Battery Power.

The following experiments were made with this apparatus (fig. 11) on July 20, 1881, to test the influence of battery arrangements upon the hearing distance of a leaden bullet.

I. *Series of experiments with a bichromate battery which had previously been in use for a few minutes.*

	Hearing distance of leaden bullet as observed by—	
	A. G. Bell.	S. Tainter.
	cm.	*cm.*
1 cell	2.4	2.6
2 cells in series	3.3	3.5
3 cells in series	3.7	4.1
4 cells in series	3.7	4.0
5 cells in series	4.0	4.1
6 cells in series	4.3	4.4
6 cells in multiple are	2.6	2.9
6 cells in two series of 3 each — | o o / o o / o o | —	3.8	3.7
6 cells in three series of two each — | o o o / o o o | —	4.3	4.0

II. *Series of experiments with a Leclanché battery of twenty cells which had been set up for about one month. It had been kept normally upon open circuit, and had only been occasionally used.*

	Hearing distance.
20 cells in series	3.3 cm.
20 cells in 10 series of 2 each . . .	3.6 cm.
20 cells in 5 series of 4 each . . .	4.1 cm.
20 cells in 2 series of 10 each . . .	3.0 cm.

Although the battery appeared to be in good condition, a close inspection showed that the connections were dirty, and that one of the zinc wires was half broken through.

The defective cell was now removed from the circuit, and the connections of all the other cells cleaned and tightened.

III. *The following experiments were then made with the Leclanché cells united in series:*

No. of cells.	Hearing distance.
	cm.
1	2.7
2	2.8
3	3.0
4	3.3
5	3.3
6	3.5
7	3.6
8	3.8
9	4.0
10	3.8[20]

No. of cells.	Hearing distance.
	cm.
11	3.8[19]
12	4.2
13	4.2
14	4.2
15	4.3
16	4.2
17	4.2
18	4.2
19	4.2

These results are graphically represented in fig. 12.

It will be observed that the hearing distance was carried nearly two-thirds as far again as at first, simply by increasing the numbers of cells employed without any other change in the arrangement. It will also be noticed that the apparatus requires to be adjusted to complete silence in order to obtain the maximum effect.

As a general result of all our experiments with voltaic batteries, we find that *it is advisable to use a battery possessing great electromotive force and slight internal resistance, and to connect the cells in series.*

[20] Balance not quite perfect.

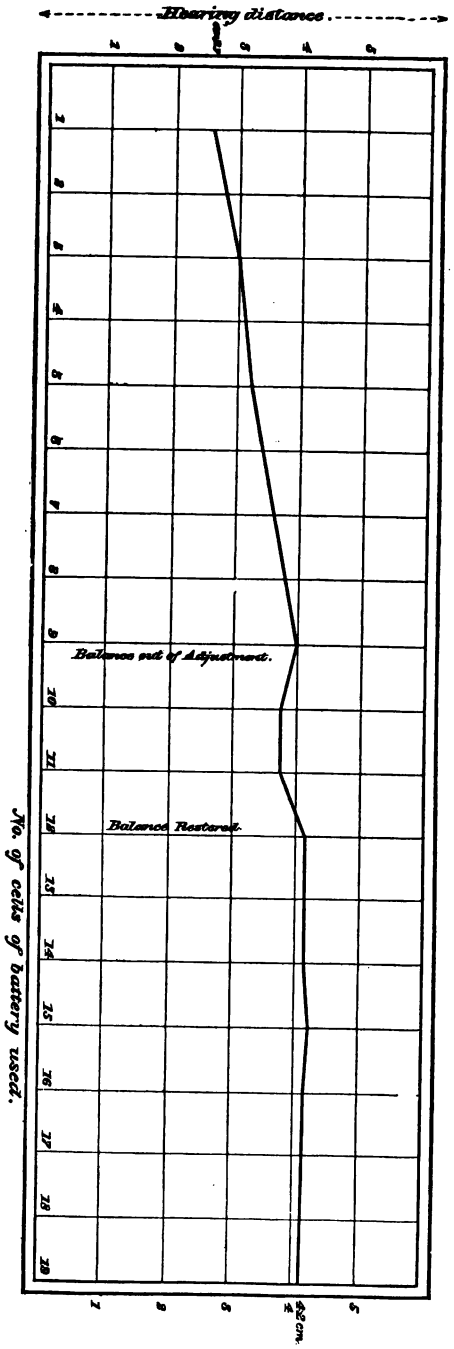

Bearing distance
Balance out of Adjustment.
Balance Restored.
No. of cells of battery used.

Fig. 12.

Experiments upon Living Subjects.

On the 22d of July an experiment was made at the request of Dr. Bliss upon the person of Lieut. Simpson, who had carried a bullet in his body for many years.

When the exploring instrument (fig. 11) was passed over the lieutenant's back a sonorous spot was found, but the indications were too feeble to be implicitly relied upon. Imagination very easily conjures up a feeble sound like that observed, but a number of experiments by different observers seemed to indicate that in this case there was an external cause for the sound—probably the presence of a very deeply-seated bullet. The results of this experiment were communicated to Dr. Bliss in a letter dated July 23, 1881.[21]

On the 25th of July Professor Rowland visited me at Washington, and suggested the use of a condenser in the primary circuit. I had previously discussed this idea with Mr. Tainter, but, not having a condenser at hand, we had been unable to make any experiment. After our conversation with Professor Rowland, however, we were so impressed by the importance of the point that we obtained a condenser next morning, and found it to produce not only a different quality of sound when the bullet approached the coils, but also to increase the hearing distance of the instrument shown in fig. 11 at least one centimetre.

On the evening of the same day (July 26) our apparatus was carried to the Executive Mansion, and an experiment tried upon the person of the President.[22]

From some cause then unknown a balance could not be obtained, and the results were therefore uncertain and indefinite. It was discovered afterwards that a mistake had been made in the mode of connecting the condenser. The latter should have been connected at E F (fig. 13), whereas it was placed at E G, thus influencing only one, instead of both, of the primary coils.

With the condenser properly arranged, experiments were tried on July 29 and 30 on three soldiers from the Soldiers Home who had been wounded during the civil war, namely, John Teahan, Asa Head, and John McGill. In the case of John Teahan no results were obtained. In the case of Asa Head, who had a buckshot in the cheek, loud and well-marked sounds were heard in the tele-

[21] See Appendix, note 12. [22] See Appendix, note 13.

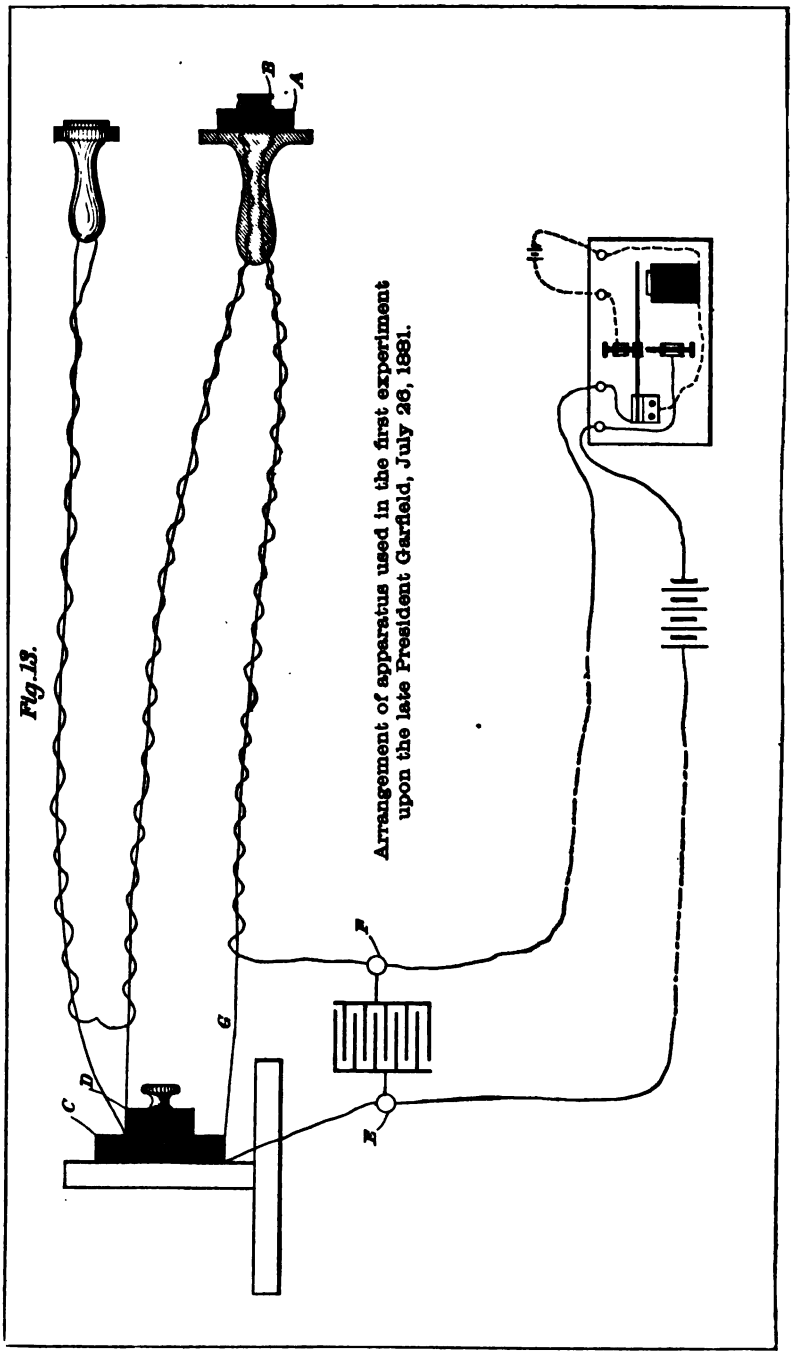


Fig. 13.

Arrangement of apparatus used in the first experiment upon the late President Garfield, July 26, 1881.

phone; and in the case of John McGill, who was supposed to carry a bullet in his back, no results were obtained.

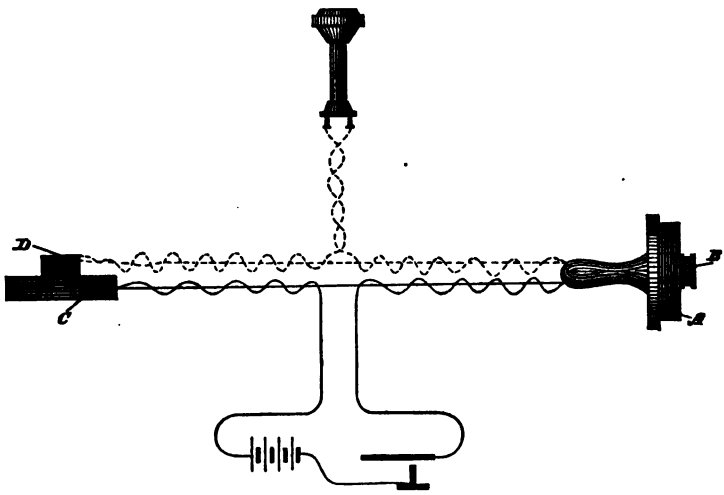
Further efforts were then prosecuted for the improvement of the apparatus.

Further Experiments to Improve Apparatus.

Our attention had hitherto been directed chiefly to modifications of the exploring instrument. We now investigated the effect upon the hearing distance, of the coils used to obtain a balance.

The following experiments, made July, 1881, bear upon the point:

Fig. 14.



EXP. 1. (See fig. 14.) Resistance of primary A of exploring instrument, 2 ohms; resistance of primary C of balancing coils, also 2 ohms; resistance of exploring secondary B, 140 ohms; and of balancing secondary D, 120 ohms.

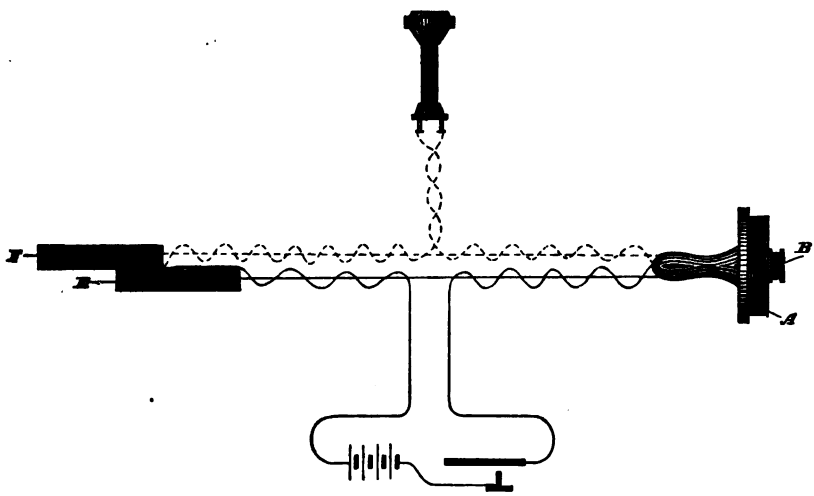
Result: Hearing distance of bullet from explorer A B, 3.5 cm. Hearing distance from balancing coils C D, also 3.5 cm.

EXP. 2. (See fig. 15.) Same exploring coils as in Exp. 1, but balancing coils consisted of a flat primary, E — resistance, 5.30 ohms; and flat secondary, F — resistance, 83 ohms. The adjustment was made by sliding the secondary coil upon the primary until a position of silence was obtained.

Result: Hearing distance from explorer A B, 1.5 cm. Hearing distance from E F, 3 cm.

As a general result of our experiments we found that *every increase in the resistance of the balancing coils (especially the primary)*

Fig. 15.



reduced the hearing distance of the exploring instrument, and it became therefore desirable to do away with this resistance as much as possible.

Return to Original Form of Apparatus.

This led us back to the original form of apparatus that had first occurred to me (see fig. 1) in which a single pair of coils was employed. A few other experiments, made July 29, 1881, will show the importance of the point attained.

Fig. 16.

Exp. 3. The two flat coils E F used in experiment 2 were arranged as in fig. 16, so as to form a balance by themselves.

Result: Hearing distance, 7 cm.

In all these experiments the battery used consisted of four cells (Leclanché).

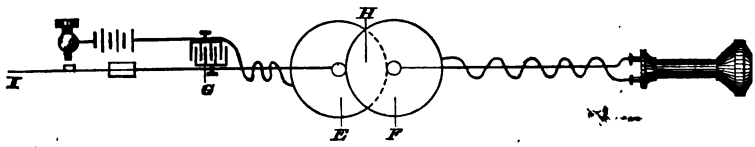
Exp. 4. The same coils used in Exp. 3 were tried again, as shown in fig. 16, but with a battery of eight cells (Leclanché).

Result: Hearing distance, 8.7 cm., or nearly $3\frac{1}{2}$ inches — a result quite unprecedented in our experiments.

The following are the dimensions of the coils E F:

Coil E.	External diameter. . . .	10 cm.
	Internal diameter. . . .	2.5 cm.
	Depth.	1 cm.
	Wire used, No. 23 (silk-covered).	
Coil F.	External diameter. . . .	10 cm.
	Internal diameter. . . .	2.5 cm.
	Depth	1 cm.
	Wire used, No. 28 (silk-covered).	

Fig. 17.



Exp. The same coils E F, used in Exps. 2, 3, and 4, were tried once more with a battery of six large bichromate elements, and with a condenser, G, in the primary circuit as shown in fig. 17.

Result: Hearing distance 13 cm., or more than 5 inches.

This great increase in hearing distance seemed to be chiefly due to the condenser, for upon disconnecting it the hearing distance was little more than 9 cm., but further experiments proved that other causes also contributed to the result.

Exp. 6. When the condenser was in circuit and the leaden bullet close to the coils (arranged as in fig. 17) the sound produced by the telephone was a musical note whose pitch was the same as that normally produced by the vibration of the reed of the interrupter. Mingled with this tone could be distinguished a number of feebler tones of very much higher pitch. Upon withdrawing the bullet gradually from the coils, the fundamental sound became fainter, and one of the high upper-partial tones gradually acquired prominence; and at a distance of about 8 or 9 cm. the funda-

mental could no longer be distinguished, but the high tone persisted, and was clearly audible up to a distance of 13 cm. The effect was very striking, and when the bullet was moved to and fro parallel to the plane of the coils E F at a distance of about 10 cm., the telephone emitted a shrill whistling sound each time the sensitive area (H) was passed.

It was noticed that other metals, such as iron, brass, and copper, did not seem to reinforce this high tone to any great extent, but brought out the fundamental at every distance where an effect was produced.

Exp. 7. The condenser G (fig. 17) was removed from the circuit and the leaden bullet held about 4 or 5 cm. from the coils E F. The fundamental tone was heard, and the characteristic upper-partial could also be distinguished, but it was only faintly audible. Upon now suddenly replacing the condenser, the high upper-partial tone was instantly reinforced as if by a resonator.

Exp. 8. The rheotome employed to interrupt the primary circuit (which had been placed in a distant room) was found to be vibrating badly. The reed I of the instrument (see also fig. 5) was rattling against its contact pieces, thus producing an impure sound, and I could distinguish amongst the upper-partials the tone that had been reinforced by the condenser. Upon screwing up the contact pieces so as to improve the vibration, I could no longer distinguish the particular upper-partial referred to, and upon returning to the room in which the coils E F (fig. 17) were placed, I could no longer detect the effects noted above in Exps. 6 and 7, and the hearing distance did not exceed 9 cm.

The peculiar effects obtained with the arrangement shown in fig. 17 *thus seemed to depend* (1) *upon a particular kind of vibration of the reed of the interrupter, producing a certain high upper-partial or overtone* (2) *upon the use of a condenser acting as a sort of electrical resonator for this tone, and* (3) *upon the use of the metal lead.*

Mr. Marean, of Washington, kindly lent me a number of condensers used by the Western Union Telegraph Co., and we found, upon connecting them with the coils E F, as shown in fig. 17, and, holding a leaden bullet near the coils, that each condenser reinforced a high upper-partial of different pitch. We arranged the condensers so that they could be successively introduced into the circuit with great rapidity. The effect was very curious, and

sounded somewhat like a Scotch air played upon the bagpipes. The low hum of the fundamental could be heard continuously, like the drone of the bagpipe, while the higher tone changed its pitch with each change of condenser.

The pitch of the high tone reinforced seemed to depend upon the electro-static capacity of the condenser employed, but the exact relation between the two has not been ascertained. In experiments 5, 6, 7, 8, and the subsequent experiments described above, the battery employed consisted of six pairs of carbon and zinc plates of large area placed in a solution of bichromate of potash containing sulphuric acid.

The effects noted above were not produced satisfactorily when the battery was much run down, nor were they obtained with a Leclanché battery which had been set up for some time, but which appeared to be in good condition.

It is evidently necessary in order to produce this characteristic high tone to use a battery possessing considerable electro-motive force and slight internal resistance.

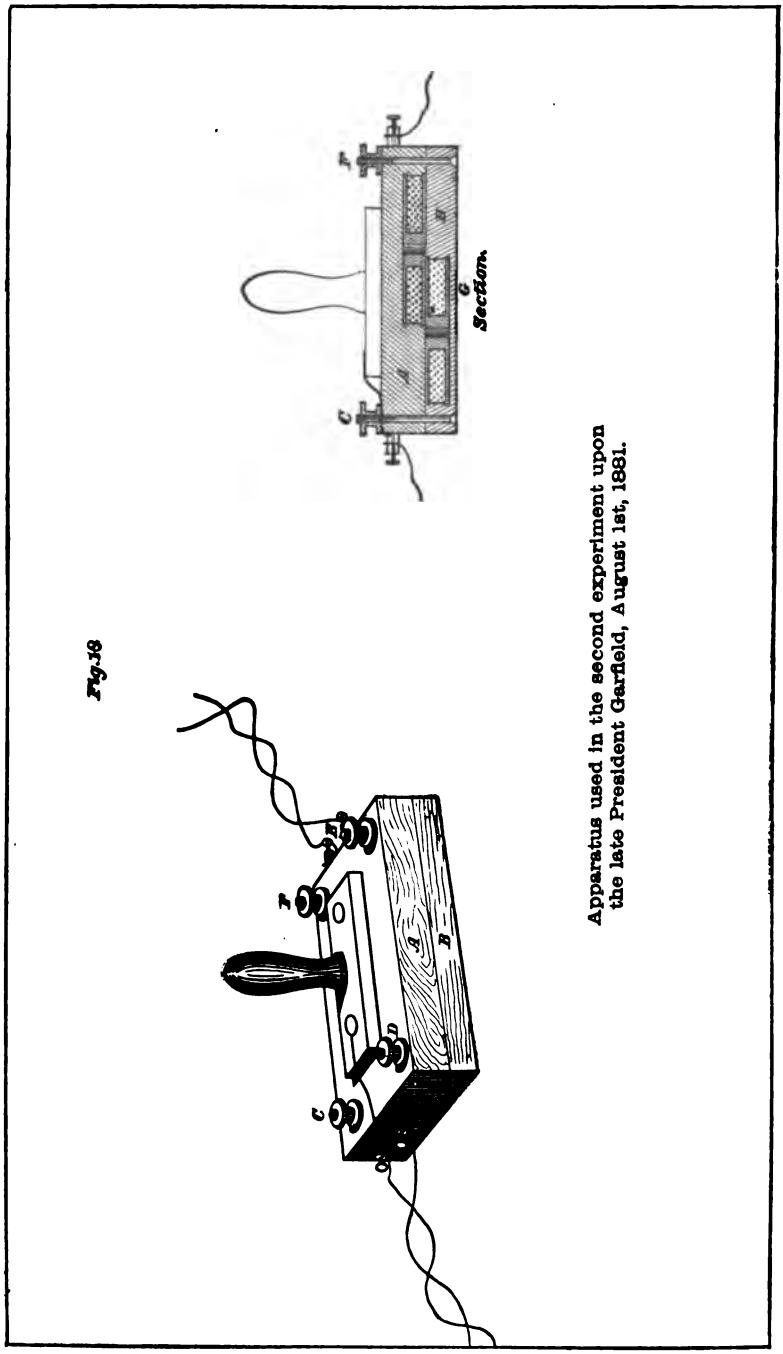
Our experiments had reached this stage when, on Saturday, the 30th of July, 1881, I was requested to make another trial upon the person of the President at the evening dressing of the wound.

At this time, however, we had no exploring instruments completed excepting one or two like that shown in fig. 11; for it will be understood that the promising results noted above had been obtained from coils that were simply placed upon a table and adjusted by hand.

We immediately proceeded to the Executive Mansion with the apparatus shown in fig. 13, prepared to make a trial, if it was deemed advisable; but upon learning of the results of our later experiments, the surgeons resolved to postpone any further trial until we could arrange the coils (fig. 17) in a portable form.

By forced exertions the coils were arranged that same night in a wooden case, as shown in fig. 18. A shallow circular recess was turned out in each block for the reception of one of the coils, and the two blocks were held together by four pins of ebonite, C, D, E, F, which passed up through slots in the upper block and were secured by ebonite thumb-screws.

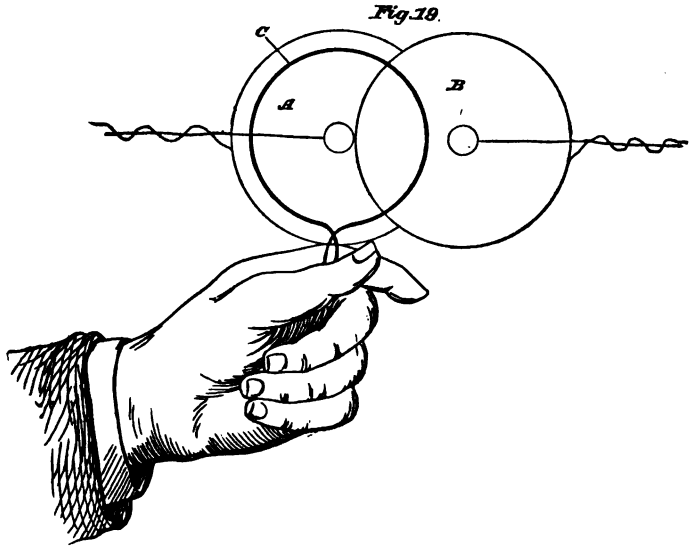
When the instrument was completed I found to my great distress that a balance could not be obtained by any adjustment of the apparatus. There was a position of minimum sound, and the tel-


Section.
Fig. 38
Apparatus used in the second experiment upon the late President Garfield, August 1st, 1881.

ephone responded to a bullet presented to the central part G of the instrument; but the hearing distance did not exceed 3 or 4 cm., whereas we had obtained with the same coils before the construction of the wooden case a perfect balance and a hearing distance of 13 cm.

After numerous unsuccessful experiments had been made to ascertain the cause of the difficulty it occurred to me that if two adjoining convolutions in one of the coils, made contact at any point, a circuit of low resistance would be formed (a single ring of wire, in fact), in which the induced currents might circulate

Fig. 19.



without reaching the telephone connected with the apparatus. I had previously measured the resistance of the coils without discovering any defect, but when I considered the large number of convolutions in each coil it seemed possible that a defect of this kind might exist which could not be discovered by a Wheatstone Bridge, excepting by very delicate and accurate observations. To test whether a short-circuited convolution would produce effects analogous to those observed, a piece of copper wire was bent into an annular form and the ends connected together. On bringing this metallic ring near a pair of coils (A, B, fig. 19), properly adjusted to silence, the balance was loudly disturbed. The copper ring (C) was held as shown in fig. 19, and the balance could not then

be restored by any adjustment of the coils. A position of minimum sound was all that could be obtained, and the hearing distance was enormously reduced. This was *prima facie* evidence of the nature of the defect.

The coils (fig. 18) were then removed from their case, but a cursory examination revealed no defect. Upon trial, however (being arranged, as formerly, in fig. 17), a balance could not be obtained, and the hearing distance was only about 4 cm. The defect was thus definitely located in the coils themselves.

Upon close examination it was noticed that the outside convolutions of the primary coil were slightly frayed at one part, but it appeared hardly possible that so great a defect could be due to so apparently slight a cause. However, to test the matter, I removed the outside layer of wires and then tested the coils again.

Result: The defect had vanished — a perfect balance was obtained, and the hearing distance was again 13 cm. [23]

The coils were then replaced in their case and the completed instrument tested. The lower wooden block B (fig. 18) was adjusted by hand as nearly as possible to the position of silence, and then the thumb-screws C, D, E, F were tightened.

The balance now obtained was not quite perfect, but by striking

[23] These experiments have revealed the cause of the extreme difficulty always experienced in obtaining a perfect balance with coils of fine wire. I have recently used an Induction Balance to test the condition of the helices that were employed in these researches, and have discovered that in a large percentage of cases the insulation was defective. It is possible that some of the results described in this paper (especially of the earlier experiments) may have been vitiated by errors due to defects in the coils that were not suspected at the time. A defect of insulation that is quite immaterial for ordinary purposes may be absolutely fatal to the success of an Induction Balance. Indeed, so much care is required in this respect that it is extremely difficult to obtain coils that are perfectly suitable for an apparatus intended to search out a bullet embedded in the body. I now make it a rule to test every helix used in Induction Balance experiments by bringing it up to a system of balanced coils like that shown in fig. 19.

1. If the helix is perfect the balance is not disturbed until the terminals of the coil are connected.

2. If there is a break in any of the convolutions the balance is not disturbed, even when the terminals are connected.

3. If a convolution is short-circuited the balance is disturbed, even though the terminals are not connected, and the sound produced is the fundamental of the rheotome employed to interrupt the primary circuit.

4. If the insulation is defective the balance is disturbed, although the terminals are not connected, and a peculiar spluttering effect is noticed like that produced by a series of sparks.

I propose to apply this method practically as a means of testing the condition of the helices used in the construction of Induction Coils and those employed in the manufacture of telephones.

the lower block B a few smart blows with a wooden mallet we were able to reduce the arrangement to complete silence.

The instrument was then in such a sensitive condition that it could scarcely be moved without affecting the balance. Upon gently swaying it backwards and forwards a pulsation of sound was heard at every swing.

When the motion was carefully made, so that it was always in the same plane, no pulsations were observed. They only occurred when the inclination of the coils was changed.

This defect was found to be due to the bulging of the thin portion G of the wooden case (fig. 18) under the weight of the enclosed coil, and the simple pressure of a finger on this portion of the case disturbed the balance. The movement of the lower coil when the instrument was swayed about must have been inconceivably small, but on account of the extreme sensitiveness of the arrangement it produced a perceptible effect upon the balance.

The pulsating sound did not seem to interfere with the detection of a bullet held in the clenched hand, nor did it seem to affect the hearing distance. I therefore despatched a messenger to the Executive Mansion (Sunday morning, July 31) with a note for Dr. Bliss,[24] to let him know that the instrument was in a condition to be used, should any necessity arise for an immediate experiment. At the same time I informed him that the apparatus in its present form was very crudely constructed, and that I hoped to improve it very greatly in the course of a few days. On Sunday afternoon (July 31) we sent to the Soldiers Home for John McGill, upon whom we had experimented the previous day without results (using the apparatus shown in fig. 11).

Upon trying the new instrument (fig. 18) we had no difficulty in finding a sonorous spot in his back, at the place where the bullet was always supposed to be.

This result was at once communicated to Dr. Bliss,[25] and in reply we were requested to make the experiment upon the person of the President next morning.

On Monday morning (August 1, 1881) we accordingly removed our apparatus to the Executive Mansion.

[24] See Appendix, note 14. [25] See Appendix, note 15.

The Late President Garfield.

During the former experiment (July 26) a sudden sonorous effect had been observed upon passing a point near the spot where the surgeons suspected the bullet to be lodged, but I had been unable to verify this by a second observation, although the exploring instrument (A B, fig. 13) was repeatedly passed over the same place. The sound had been so loud and well marked that I believed at the time it must have been caused by a sudden irregularity in the vibration of the reed of the rheotome used to interrupt the primary circuit, for the arrangement (as explained above,) was not perfectly balanced, and any irregularity of this kind would, under these circumstances, have affected the telephone. At the same time the coincidence was remarkable that the exploring instrument should have been at that very time so near the suspected seat of the ball, and this led to the thought that perhaps after all the bullet had been the cause of the sound. I felt confident that the new instrument (fig. 18) would at once decide the question, for the extreme hearing distance of the former apparatus (fig. 13) was only 6 cm., and the apparatus shown in fig. 18 was so superior in this respect that if the sound had really been due to the bullet we should obtain with the new instrument distinct and well-marked effects. When the new explorer (fig. 18) was passed over the suspected spot nothing was heard excepting a slight pulsating sound as the instrument was moved to and fro. This was evidence to me that the former sound had been of accidental origin, whether the bullet was there or not. With a view of eliminating any error of observation caused by the pulsations due simply to the movement of the instrument, I lifted the latter (without changing the inclination of the coils) to a height of about fifty centimetres above the body of the President, and moved it to and fro in as nearly as possible the same way I had done at the lower elevation.

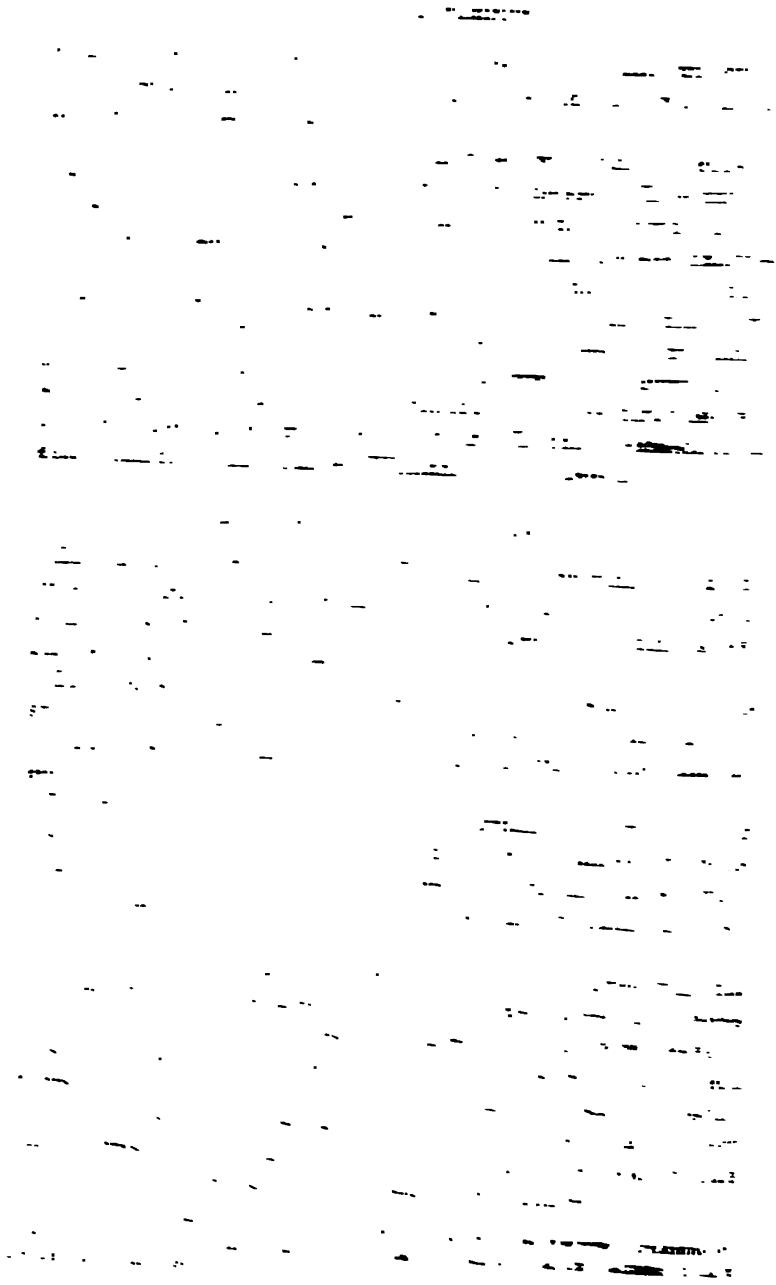
I presumed that if the pulsations heard were due simply to the movement of the instrument, they should occur with equal strength at the two elevations; but if any portion of the sonorous effect was due to the influence of the bullet, the pulsations at the two elevations would be different in intensity. I was struck by the fact that, although the sonorous pulsations were very feeble, they were sensibly louder when the instrument was close to the surface of the body than when it was raised. Continuing the exploration, I

found a considerable area over which similar effects were noticed, but upon carrying the instrument towards the back of the President, the difference between the pulsations produced at the two elevations grew less and less, and finally could not be distinguished.

The difference in the loudness of the sound at the two elevations was so slight that it probably would not have been noticed by an ear unaccustomed to listen to feeble effects, and I feared that the general expectation that the bullet would be found in that part of the body might have led me to imagine a difference that did not exist. For the purpose of eliminating as far as possible any personal error, I requested Mr. Sumner Tainter (who was the only other person present whose ear had been sufficiently trained to be reliable in such an emergency) to repeat the experiments and let me know the result. Upon our return to my laboratory we compared notes, and I found that his observations tallied with mine. He declared he could not obtain a distinctly localized effect, but stated that he had observed a reinforcement of the pulsation over an area of at least two inches in the neighborhood of the spot to which his attention had primarily been directed, and that he was convinced that the bullet was within that area.

It appeared reasonably certain that the area of feeble sound was due to some external cause, and was not simply an effect of expectancy. In the absence of any other apparent cause for the phenomenon I was forced to agree in the conclusion that it was due to the presence of the bullet, and I so stated in my report to the surgeons.[26] I was by no means satisfied, however, with the results obtained, for no such effects had been observed before in our experiments with bullets. I tried to reproduce the effects by moving the instrument (fig. 18) at different distances over a bullet, but in every case where an effect was produced the sound was quite sharply localized. I thought that perhaps the body of the patient might have affected the result, and so experimented upon a bullet buried in a piece of meat, but no difference of effect was noted. This led me to fear that the extensive area of feeble sound might have been due to some extensive area of metal that was unsuspected at the time, and I proceeded to the Executive Mansion next morning (August 2) to ascertain from the surgeons whether they were perfectly sure that all metal had been removed from the

[26] See Appendix, note 16.



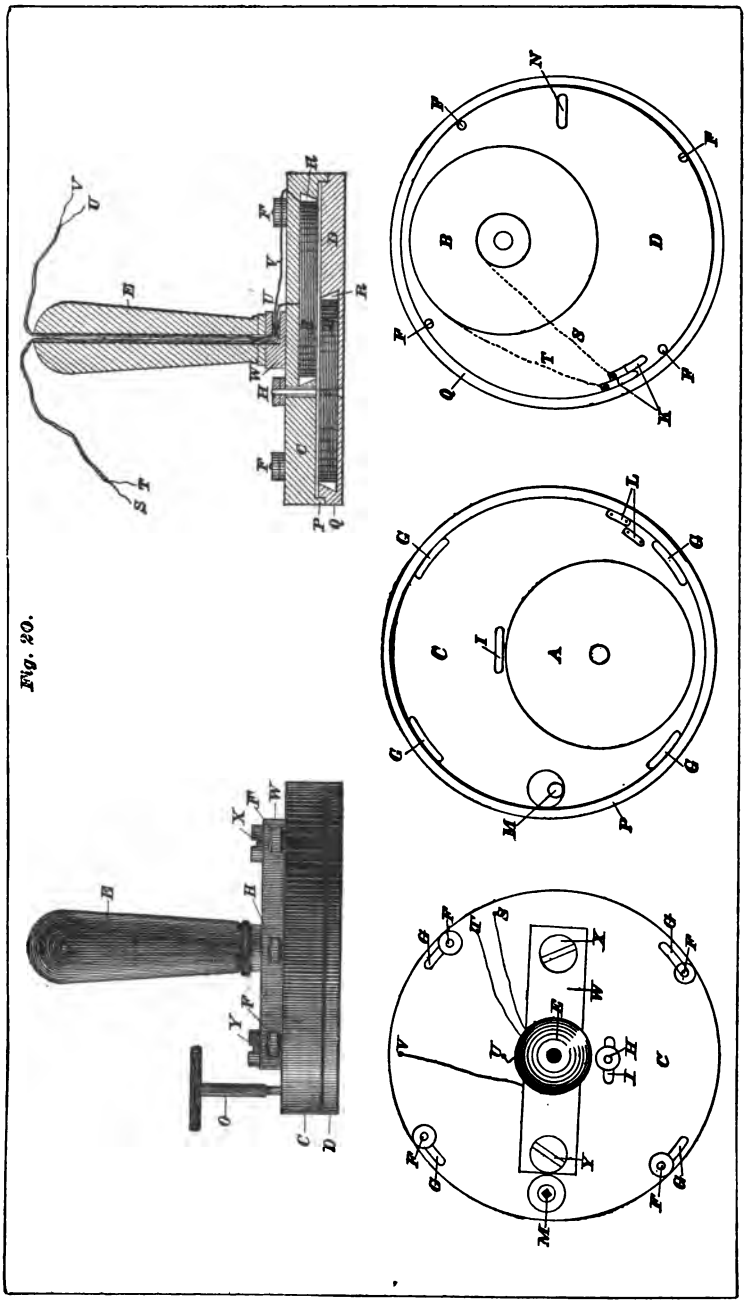


Fig. 20.

neighborhood of the bed. It was then recollected that underneath the horse-hair mattress on which the President lay was another mattress composed of steel wires.

Upon obtaining a duplicate, the mattress was found to consist of a sort of net of woven steel wires, with large meshes. The extent of the sonorous area having been so small, as compared with the area of the bed, it seemed reasonable to conclude that the steel mattress had produced no detrimental effect.[27] I was unable to continue experiments with the steel mattress, as just at this time I was obliged to leave Washington on account of illness in my family. Although I was unable for a long time afterwards to carry on personally Induction Balance experiments, the investigations were ably continued under my direction by Mr. Thomas Gleason, in the establishment of Mr. Charles Williams, Jr., in Boston.

Experiments continued in Boston.

Mr. Tainter forwarded from Washington drawings of an improved apparatus he had designed, to remedy the defects of the instrument shown in fig. 18, in which the case, adjusting screws, etc., were all to be composed of ebonite.

Mr. Gleason constructed for me a number of such ebonite instruments differing slightly from one another in detail, and the apparatus shown in fig. 20 combined the different points that had been approved.

The two coils A B were eccentrically arranged in two circular disks of ebonite, C D, and the adjustment was obtained by means of an ebonite key O, like the key used for tuning pianos, which turned a cam M pivoted in the upper disk and working in a slot N in the lower disk.

In order to prevent any movement of the coils, excepting that produced by the adjusting-key O, each coil was placed in a recess turned out in its ebonite disk, the edges of which were bevelled as shown at R. Paraffine was then poured in so as to fill up each recess. But this alone did not prevent a slight pulsation of sound when the instrument was swayed from side to side, and a very slight pressure of the finger on the thin portion of the ebonite plate under the coil B was sufficient to destroy the balance.

[27] The death of President Garfield and the subsequent *post-mortem* examination, however, proved that the bullet was at too great a distance from the surface to have affected our apparatus.

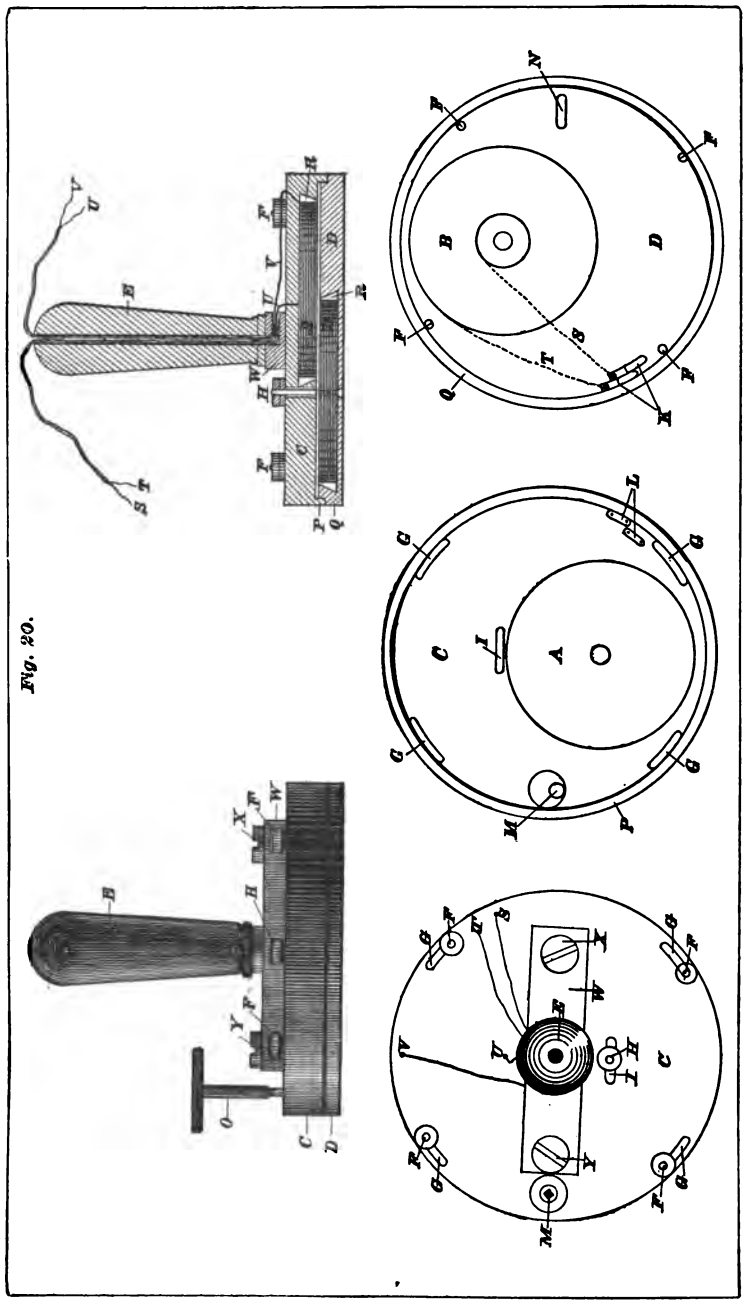
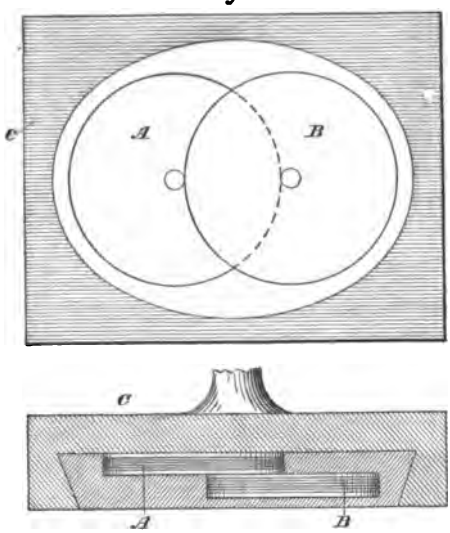


Fig. 20.

This was remedied by strengthening this portion by means of a rod of ebonite, which passed up through the centre of the coil and through a slot I, in the upper ebonite plate, and was clamped firmly after the adjustment of the instrument by an ebonite thumb-screw H. This, however, increased the difficulties of adjustment. When the coils were adjusted to silence, then the tightening of the thumb-screw H disturbed the balance; and if the thumb-screw H was tightened first, then the adjustment could only be made by a series of jerks, on account of friction. In practice we found it best to adjust the instrument *almost to silence*, and then the tightening of the thumb-screw H completed the balance.

Fig. 21.



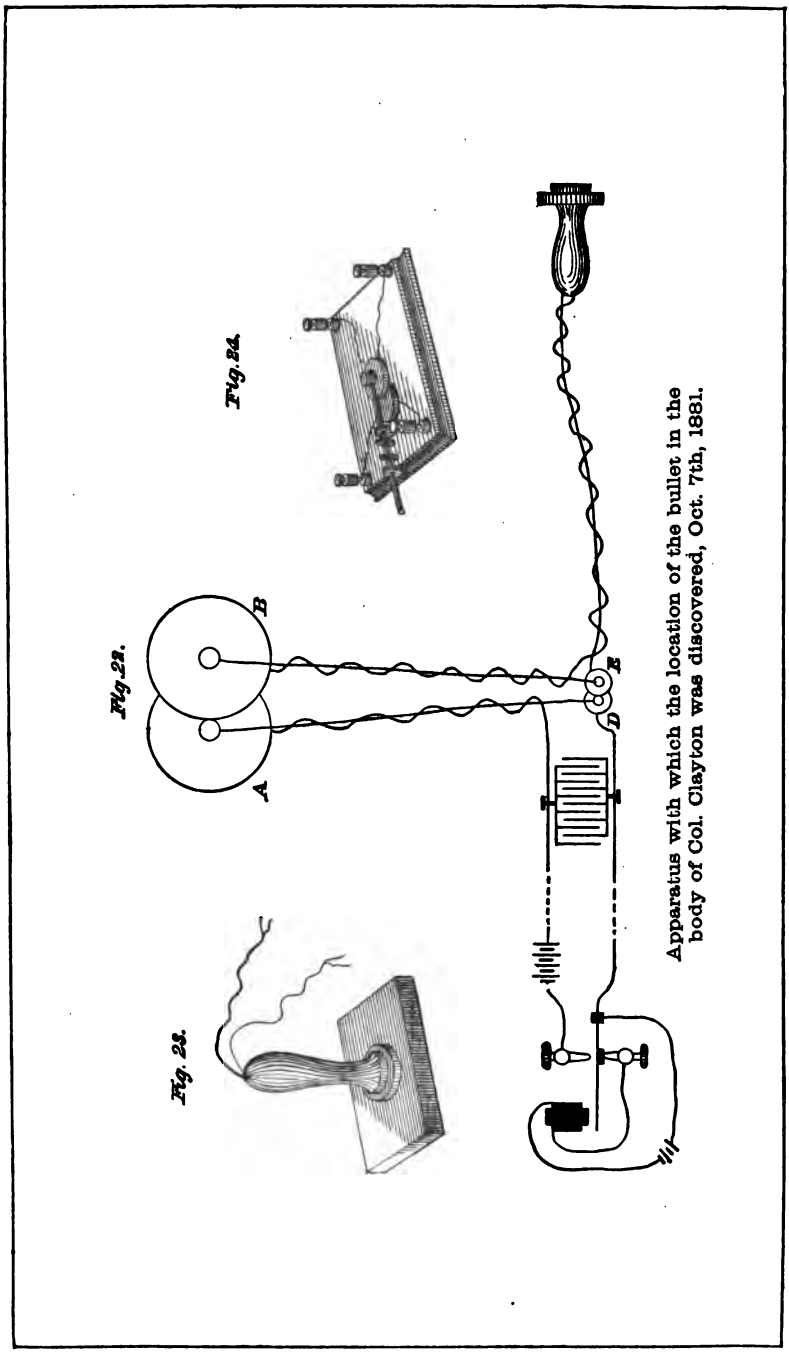
This was the form of apparatus at which we had arrived at the time of the death of President Garfield.

The difficulty of adjusting the coils led me ultimately to the idea of the apparatus shown in figs. 21, 22, 23, 24, which is the most practical form of the instrument yet devised.

The two exploring coils A B (fig. 21) are arranged as shown, in a recess turned out in a single block of wood C.

The coils are temporarily connected with a telephone, battery and rheotome in the manner shown in fig. 1, so that they may be adjusted by hand to form a balance. When they have been arranged in their position of silence the hollow in the block of wood C (fig. 21) is filled with melted paraffine. Upon cooling, the two coils are found immovably fixed in one solid cake of paraffine.

As a matter of practice it is found impossible to fix the coils in this way exactly in their position of silence; but by means of two other very small coils, D E (fig. 22), of insignificant resistance, forming a sort of fine adjustment external to the explorer, a per-



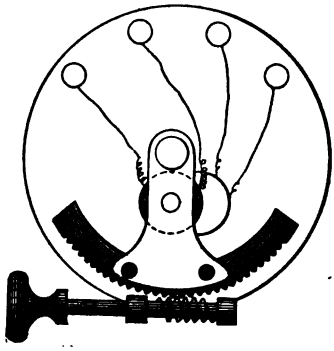
Apparatus with which the location of the bullet in the body of Col. Clayton was discovered, Oct. 7th, 1881.

fect balance is easily obtained. In this instrument the swaying of the coils A B produces no effect upon the balance.

The completed arrangement is shown in plan in fig. 22, and the explorer and balancing coils are shown separately in perspective in figs. 23 and 24.

On account of the small size and slight resistance of the balancing coils we were enabled to make the adjustable parts of the balancer of metal without practical interference with the sensitiveness of the exploring instrument, and this gave us the power of making very delicate adjustments of the balancing coils.

Fig. 25.



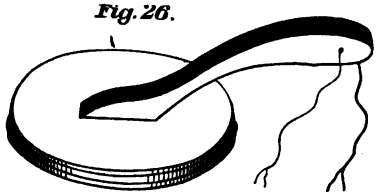
We found it advisable, however, to avoid placing metal over the sensitive area of the coils as had been done in the instrument shown in fig. 24.

In the balancing apparatus shown in fig. 25 (which is the most perfect one yet constructed), the lever to which the upper coil is attached is made of hard rubber.

In fig. 26 is shown the most convenient form of case yet devised for holding the exploring coils.

By invitation of Dr. Frank Hamilton, experiments were made at his office in New York, October 7, 1881, the instruments used being those shown in figs. 22, 23, 24.

Fig. 26.



As this was the first successful application of the Induction Balance to the discovery of the situation of a ball in the body, the position of which was previously unknown, I may be pardoned for entering somewhat into detail.

I shall quote from the *Medical Gazette*[28] of New York, an account of the experiments written by one of the witnesses:

[28] See *Medical Gazette*, Oct. 15, 1881, pp. 347-349.

"*The First Successful Application.*

On Friday, Oct. 7, by invitation, several medical gentlemen,[29] including the writer, met Prof. Bell at the house of Dr. Frank H. Hamilton, in this city, for the purpose of witnessing the practical application of his improved instrument.

The first person subjected to experiment was General Calvin E. Pratt, judge of the supreme court of the State of New York, and who is now a resident of Brooklyn. General Pratt, at the battle of Gaines' Mills, June, 1862, this being the second day of the famous seven days' retreat across the peninsula, received a ball in his left cheek, which penetrated through the nares and was lodged in the right antrum. Its presence at this time was recognized by his surgical attendant, Dr. Damainville, and its exact position has been known from that day until this, it having given rise at times to much pain and suffering.

General Pratt has been seen by Dr. Hamilton and Dr. Damainville occasionally from that time forward, and they have from time to time urged upon him the necessity of its removal. General Pratt, however, was anxious to know whether Professor Bell's instrument would indicate its presence at the same point as declared by his surgeons.

The results of the experiment were conclusive and entirely satisfactory to General Pratt, the response being heard distinctly, but rather feebly, by every person present in the room. The feebleness of the response was supposed to be due to the fact that, owing to its situation and the peculiar form of the instrument containing the induction coils, it was impossible to bring the centre of its surface very near the site of the ball, the ball being situated very near the depression at the ala of the nose.

The next patient was Col. B. F. Clayton, who received a wound at the battle of Cedar Mountain, Virginia, Aug. 9, 1862.

The missile was supposed to be an Enfield rifle ball, and the wound was supposed to be mortal by the medical director of General Banks' staff and his assistants. The ball passed through the

[29] "The following are the names of the medical gentlemen who were present, each one of whom verified personally the results and declared his entire satisfaction with every experiment that was made: J. C. Hutchinson, J. G. Johnson, and J. G. Allen, of Brooklyn; Elias Marsh, of Patterson, N. J.; Nathan Bozeman, J. H. Hunter, G. Durant, F. Delafield, L. Damainville, W. M. Chamberlain, J. H. Girdner, Frank H. Hamilton, and E. J. Bermingham, of New York."

sternal end of the left clavicle, and was supposed to have lodged in the muscles under the superior angle of the corresponding scapula. The injury was followed by complete paralysis of the left arm, continuing for a period of six months; and his arm has never yet been completely restored to its normal condition. He suffers a great portion of his time from pains in the arm, shoulder, and portions of the back.

Several small fragments of bone escaped through a fistulous orifice formed near the seat of the original wound.

About eighteen months later an abscess opened on the front of the chest below the fifth rib and to the left of the sternum. Through this sinus his surgeon was able to carry a probe upwards and backwards towards the top of the shoulder several inches, and which sinus was supposed then to communicate with the seat of the ball on the back.

Pleural adhesions were recognized by the medical attendants as having occurred in the upper part of the left thoracic cavity. He has been troubled occasionally ever since the injury with cough, expectoration, and violent palpitations of the heart. A suspicion has even been entertained that the fistulous canal which remained open a period of eighteen months, and then became permanently closed, communicated with the bronchial tubes, but at no time was a suspicion entertained by him or his medical attendants that the ball was not lodged in the back and there closely encysted.

We are disposed to mention as an evidence of Col. Clayton's loyalty and faithfulness as a soldier that within six months of the receipt of the injury, and while the wound was still discharging pus and blood, he returned to active duty with his regiment and remained in the field until the close of the war.

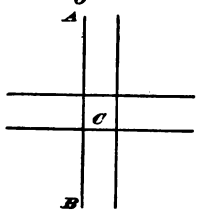
In the presence of the gentlemen assembled Col. Clayton exposed his chest, and Professor Bell proceeded to move the coils across that portion of his back where the ball was supposed to be situated, the colonel indicating the point underneath the superior angle of the scapula as that which had been fixed upon by himself and all the surgeons who had examined him as its exact seat. Although being buried underneath the scapula, they had not been able to verify their diagnosis by the sense of touch. Repeated examinations were made over this region, without any response, both by Professor Bell and several of the gentlemen who were present.

The instrument was then moved in every direction across the back and shoulders with the same result. There was an evident feeling of disappointment on the part of Professor Bell and all the gentlemen present, for no one entertained a doubt up to this moment that the situation of the ball was known and correctly stated by Col. Clayton.

It was not until the lapse of half an hour, and a thorough examination on the part of Professor Bell to determine if there was not some imperfection in the working of the apparatus, that it was suggested to move the instrument along the front of the chest.

This was done by Professor Bell, and immediately he exclaimed: 'I have found it!' And such was evidently the fact, as was verified by the personal examination through the telephone by every gentleman present. The response when the instrument was moved over the seat of the ball was loud and distinct, and left no room for doubt."

Fig. 27.



After all the visitors present had had the opportunity of verifying my discovery of the sonorous spot on the chest of Col. Clayton, experiments were made to determine as accurately as possible the exact position of the ball.

The exploring instrument (fig. 23) was first held over that part of the chest where the maximum sound was obtained. The instrument was then moved slowly towards the left until the sound could no longer be perceived. The position of the centre of the instrument was noted, and a vertical line (A B, fig. 27) was drawn with ink upon the skin through that point. This line indicated the boundary of the sonorous area towards the left. The experiment was then repeated by moving the instrument from the point of maximum sound towards the right, and also upwards and downwards, giving us the four boundary-lines shown in the diagram. (fig. 27). The bullet was thus located within a square, C, of about one inch.

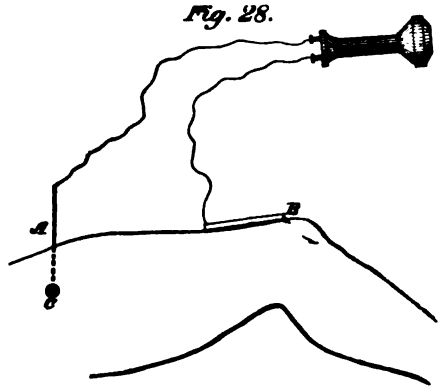
"The exact position of the ball," as described in the *Medical Gazette*, "was found to be within the thorax, probably in immediate contact with the inner surface of the ribs, the point being a little to the left of the sternum, between the third and fourth ribs.

and two or three inches above the cicatrix on the front of the chest, where the sinus, long since closed, had evacuated itself, and in a direct line from this cicatrix towards the left shoulder, which indicated the line of the track of the original sinus."

Experiments with Needles.

During my absence from Washington and from all conveniences for experimenting personally with Induction Balance apparatus, I devised a method of verifying the indications of the Induction Balance and of ascertaining the exact depth at which a bullet lies beneath the surface. This method was communicated through Dr. Woodward to the surgeons in attendance on President Garfield, and it was made the subject of a special paper presented to the French Academy of Sciences, Nov. 7, 1881.

Fig. 28.



This method, although involving extremely slight pain, would ordinarily be used only as a preliminary to an operation for the extraction of a bullet. The arrangement is shown in fig. 28. A fine needle A is connected to one terminal of a telephone, and the other terminal makes contact with a plate B, preferably of the same material as that composing the needle. Place this metallic plate B against the surface of the patient's skin and thrust the needle into that portion of the body where the bullet is believed to be lodged. When the point of the needle makes contact with the surface of the bullet C, a galvanic battery will be formed naturally within the body, the two poles of which are respectively the leaden bullet C and the metallic plate B. Under these circumstances a click will be heard from the telephone each time the bullet is touched by the needle. This has been verified by experiments upon bullets buried in a joint of meat. The click, though feeble, is unmistakable.

I have no doubt that this method of exploration alone, without the induction balance, would prove of great service upon a field of battle, where the employment of complicated apparatus is impossible. Mr. Thomas Gleason has recently communicated to me the particulars of an experiment he witnessed, in the course of which this method was tried upon a living subject. The surgeon who conducted the experiment was unable to obtain any response from the induction balance employed, although from certain indications apparent to the sense of touch he believed that the bullet was located in the part of the body submitted to experiment.

To verify his supposition a needle connected as above (fig. 28) was thrust into contact with the hard substance perceived, but no response was made by the telephone. The surgeon, however, believing that the bullet had been found, etherized his patient and proceeded with an operation, but discovered, when too late, that the bullet was not there.

Further Modifications of Induction Balance.

I sailed for Europe early in October, 1881, and have had no opportunity since of continuing my researches until quite recently. While I was in Europe, however, Mr. Sumner Tainter devised a new kind of induction balance which deserves mention here. The results obtained with this apparatus in its present form (fig. 29) are not to be compared with those produced by the best instruments described above, but there are undoubtedly great possibilities of future development.

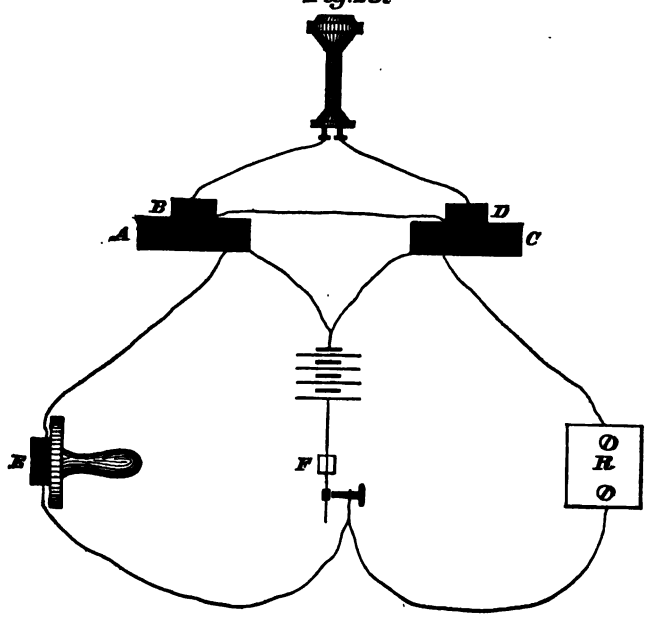
The important feature is that the exploring instrument E consists of *a single coil*, so that there is no possibility of any part of the explorer getting out of adjustment. All the adjustments are made upon the stationary part of the apparatus.

The current of the battery is divided between two equal circuits. One of the primary circuits contains the coil A and the exploring coil E, and the other circuit the coil C and a rheostat R. Coils A and C are exactly similar; and if the resistance introduced at R is equal to the resistance of the exploring coil E, an acoustic balance can be obtained by the adjustment of the secondary coils B D upon the primaries A C; but if the resistance introduced at R is different from that at E, Mr. Tainter states that no balance is possible.

When the apparatus is adjusted to silence the approach of a bullet to the coil E destroys the balance.

Although the great object of the researches that have been brought before you to-day has been to find that arrangement of balance which will detect a bullet at the greatest distance from the coils of the explorer, it must not be forgotten that in every case the instrument is more sensitive to the presence of a bullet placed *inside* the exploring coils than to one exterior to them. When, therefore, we seek the location of a bullet in one of the limbs, it

Fig. 29.



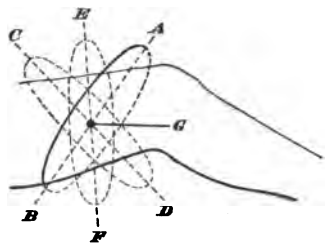
may be advisable to use an annular coil large enough to slip easily over the leg or arm, as the case may be.

In Mr. Tainter's arrangement the exploring coil E (fig. 29) might simply be a large ring consisting of a number of convolutions of thick wire which could be slipped over the limb, or the ring might consist of two coils, forming one side of a Hughes' induction balance.

In either case the loudest sound will be produced when the bullet is in the plane of the ring, and its exact location should be deduced from three observations. Suppose, for instance, that with the ring inclined in a particular direction the maximum sound is observed when the ring occupies the position A B (fig. 30). We know then

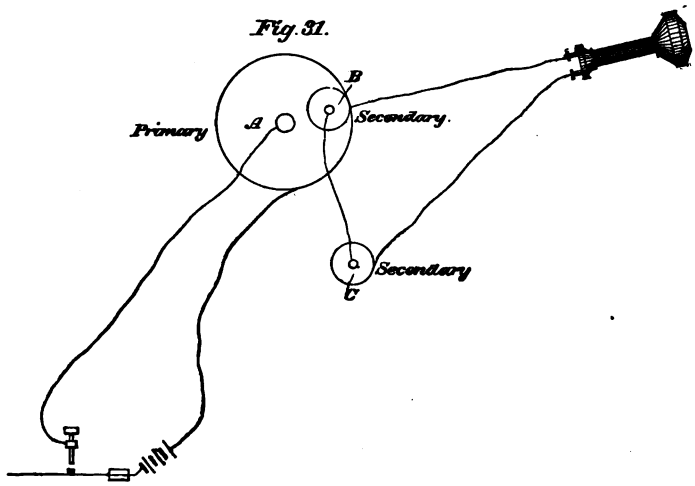
that the bullet is in that plane. Now, incline the ring in some other direction and explore again. Let the position of maximum sound be now C D. We know then that the bullet is somewhere on the straight line formed by the intersection of the planes A B and C D. It is only necessary then to make a third observation with the apparatus so inclined that the plane of the ring cuts this straight line, for instance, the position E F. The point of intersection of the three planes G is then the exact point occupied by the bullet.

Fig. 30.



I shall conclude this paper by the description of an experiment made in Newport, R. I., a few days ago. The results are so unprecedented in my experience that I feel they cannot be received as implicitly reliable until the experiments have been repeated and verified.

Fig. 31.



I had arranged upon a table three coils (as shown in fig. 31). The large flat primary coil A was connected with a battery of four Bunsen elements and an interrupter, as shown, and the two small secondaries of fine wire, B C, were connected with a telephone.

The secondary B was moved about on the primary A until a position of silence was obtained. Upon bringing a leaden bullet near C the balance was disturbed and a distinct sound produced from the telephone. There is nothing very strange about this when we know that the distance between A and C was only fifteen centimetres; so that C was well within the field of induction of A; but what did seem extraordinary was that the approach of the large steel blade of a penknife to the coil C produced no effect! The iron diaphragm of a hand telephone brought close up to the coil C produced no sensible disturbance of the balance, whereas a small disk of lead produced quite a marked effect. A disk of copper the size of a telephone diaphragm also produced a good effect, but the sound was not sensibly louder than that due to the small leaden disk. A diaphragm of zinc occasioned a feeble, but distinct, disturbance of the balance. It is unfortunately the case that in all the forms of induction balance described above, lead gives the poorest effect of all metals. If people would only make their bullets of silver or iron there would be no difficulty in finding them in any part of the body! In the apparatus shown in fig. 31, however, it seems (unless subsequent experiments should reveal some fallacy) that we have an arrangement which is sensitive to lead and not to iron, or, at all events, which is more markedly influenced by lead than iron.

It is hardly necessary to state that when the coil C was removed to a considerable distance from the primary A, no effect was produced by the approach of metal to the coil C.

I have in this paper brought before you an outline of a labor of love pursued through many anxious days and sleepless nights. However imperfect or disappointing may be the results so far achieved, they are sufficiently encouraging to enable us to look forward with confidence to the attainment of still greater perfection.

I hope to continue these researches in the future; and certainly no man can have a higher incentive to renewed exertion than the hope of relieving suffering and saving life.

APPENDIX.

NOTE 1. — Another mode of painless exploration suggested itself to my mind at this time, based upon the fact that a leaden bullet is much more opaque to light than the substances composing the human body.

I was aware of the fact that the ingenious M. Trouvée, of Paris, had, by means of his polyscope, produced a light inside a living fish, and was thus enabled in the dark to see the anatomical structure of the fish as it swam about in a vessel of water.

M. Trouvée had himself shown me a modified form of this apparatus for the illumination of the interior of the human stomach, and I understood him to say that when the instrument was used the body of the patient could be seen in the dark, faintly illuminated, like a Chinese lantern, and that the extent and location of tumors in the stomach could be detected on account of their great opacity.

It occurred to me that leaden bullets were certainly more opaque than tumors, and that a painless method of exploration might be based upon the observations of M. Trouvée.

It would evidently be impracticable, in the great majority of cases, to introduce into the stomach any illuminating apparatus; but if the light of a properly protected incandescent platinum spiral, introduced into the stomach, could produce effects visible from the outside — that is, if this feeble light could penetrate through the substance of one-half of the body — then why should not the intense light of an electric arc, or of a lime light, penetrate the whole body from one side to the other so as to produce similar effects?

The most feasible plan that occurred to me was to place an electric lamp at one end of a long opaque tube, and to apply the other end closely against the skin of the patient. The interior of the tube between two plate-glass diaphragms could be filled with a saturated solution of alum, or some other highly-absorbent transparent substance, so as to obstruct the passage of heat rays.

Of course, the whole apparatus was to be so arranged that no light from the lamp could escape into the room to interfere with the experiment.

Under these circumstances, the body of the patient, or at least a portion of it, should, in a dark room, appear self-luminous; and it seemed possible that the shadow of an embedded bullet might be projected upon the skin. The track of the bullet might also, perhaps, be discernible as an illuminated streak either more or less bright than the surrounding surface.

These considerations led me to try a number of experiments, which proved that the method was feasible where the bullet was very near the surface of the body. On account of the great and irregular refraction of light in passing through the tissues of the body, I doubt whether the shadow of a deeply-seated small bullet could be distinguishable upon the skin, unless, indeed, a very brilliant light emanating from a single point could be safely produced inside the body.

The following experiments bearing upon the subject were made in Boston, July 6, 1881, with the assistance of Mr. Wm. Schuyler Johnson:

Experiment 1.— Mr. Johnson placed in his mouth a glass cylinder containing a spiral of platinum wire, which was rendered incandescent by the passage of an electrical current. He also held in his cheek a small leaden bullet.

In a dark room the effect was very striking. The cheek appeared semi-transparent, and the location of the bullet was detected at a glance by a shadowy spot upon the skin.

Experiment 2.— A metallic glove button (not more than 4 or 5 mm. in diameter) was attached to the glass cylinder by a few turns of black thread passed round the cylinder.

In this case the effect, although not so marked as in the former experiment, was quite discernible, and a faint shadowy streak could be seen crossing the cheek, caused by the shadow of the black thread tied round the cylinder.

NOTE 2.— *Letter from Mr. George M. Hopkins, published in the New York Tribune, July* 11, 1881.

LETTERS FROM THE PEOPLE — THE BULLET FIRED BY GUITEAU.

A suggestion that the Induction Balance be used to discover its position in the President's body.

To the Editor of the Tribune:

SIR: The attempt upon President Garfield's life and the present condition of the sufferer have called forth the nation's deepest sympathies, and elicited very many suggestions as to the methods of promoting his comfort and assuring his recovery.

As one crisis after another has passed hope has risen, and the people wait with earnest desire for the last danger to pass away.

The attending and consulting surgeons state that it is impossible to predict the final result of the shot without locating the ball.

With all deference to the several methods proposed, I desire to suggest a method of ascertaining the position of the missile which will be painless and harmless, and which, in my experiments under conditions analogous to those under which it might be practised in the present instance, has proved successful. I refer to the use of the Induction Balance — a most delicate electrical instrument for detecting the presence of metals — a modified form of which could be easily applied in this case with a reasonable expectation of success. This instrument consists of two short glass cylinders, around each of which are wound two parallel coils of fine insulated copper wire.

One coil of each pair is included in a battery circuit, in which there is a clock microphone. The other pair is placed in a closed circuit with a receiving telephone. The two glass cylinders, with their encircling coils, may be widely separated.

The induction set up in the secondary or telephone circuit is balanced

by the reversal of one of the secondary coils, and so adjusted that the induction in one of the secondary coils exactly balances or neutralizes the induction in the other, so that when the ear is applied to the receiving telephone no sound is heard.

Now, by placing ever so small a piece of metal in one of the glass cylinders, the electrical balance is disturbed and the clock on the microphone is heard to tick loudly, thus indicating the presence of metal, and the same is true if the coil be placed in the vicinity of a piece of metal.

It occurred to me to try the effect of a lead bullet upon the instrument, placing it at different distances, and separating it from the coil by insulating material. The result exceeded my anticipations, as with a set of coils that were by no means sensitive I was able to locate the bullet with the coils raised a vertical distance of nearly two inches. With more sensitive apparatus it is more than probable that the bullet might be located, even though distant several inches, by passing a pair of coils over the President's back and abdomen; and by comparative tests the depth of the bullet might be ascertained.

GEORGE M. HOPKINS.

BROOKLYN, *July* 10, 1881.

NOTE 3.— *Letter from Mr. G. M. Hopkins to Private Secretary Brown, enclosing printed description of Hughes' Induction Balance.*

OFFICE OF THE SCIENTIFIC AMERICAN, NO. 37 PARK ROW,
NEW YORK, *July* 11, 1881.

Mr. J. STANLEY BROWN,

Executive Mansion, Washington, D. C.:

MY DEAR SIR: I send herewith a full description of the Induction Balance as promised in my note of yesterday. I will send the apparatus for trial if desired.

I am certain that the bullet can be located with it if it is not too deeply seated.

Please advise Dr. Bliss.

Very respectfully,

GEO. M. HOPKINS,
of the Scientific American.

NOTE 4.— *Letter from Mr. Geo. M. Hopkins to Private Secretary Brown accompanying the Hughes' Induction Balance apparatus he forwarded to the Executive Mansion for trial.*

60 IRVING PLACE, *July* 13, 1881.

Mr. J. STANLEY BROWN:

DEAR SIR: I give below a few suggestions in regard to the use of the Induction Balance sent herewith.

Very respectfully,

GEO. M. HOPKINS.

Suggestions: Connect A to A, B to B, etc., as marked on the wires. Strength of current used, 15 or 20 volts.

A strong current extends the influence. Use the clock interrupter, and place a drop of mercury in the vulcanite cup to make connection.

The coils are now adjusted to "silence," but will probably have to be readjusted on their arrival in Washington; as a $\frac{1}{100}$ inch movement of one of the coils will throw it out of adjustment, it will be seen that the adjustment is a matter of great nicety. Two telephone receivers should be employed, and the most acute sense of hearing is required to distinguish the ticking when the ball is two inches distant from the coils.

The shorter instrument is intended for application to the patient should it be considered sufficiently sensitive to warrant the experiment.

As a preliminary experiment pass the induction balance over the pocket containing coins, keys, etc.; also over buttons, buckles, etc., attached to the garments.

Lead is the poorest of all metals to locate with this instrument. If the ball were of iron it could be readily found at a distance of 3 or 4 inches.

NOTE 5.— *Telegram from Professor Newcomb.*

EXECUTIVE MANSION,
WASHINGTON, D. C., *July* 13, 1881.

To Prof. A. G. BELL,

Care of Chas. Williams, 109 *Court St.*, *Boston*, *Mass.*:

Perhaps small core for coil no larger than bullet, with very fine wire, might give best effect. Shall telegraph to Rowland to know best theoretical form. Telegraph me or Brown, care Executive Mansion, what train you take.

S. NEWCOMB.

NOTE 6.— *Telegram from Prof. John Trowbridge.*

BAR HARBOR, MAINE, *July* 16, 1881.

To Secretary Brown,

White House, Washington, D. C., for Professor Bell:

Make resistance of secondary coils equal to telephone. Put large number of turns of wire on primary and secondary coils. Primary coils long compared with width. Put secondary coils around middle of primary coils.

JOHN TROWBRIDGE.

NOTE 7.— *Telegram and letter from Professor Rowland to Professor Newcomb.*

HUNTER, N. Y., *July* 14, 1881.

To Prof. SIMON NEWCOMB,

Executive Mansion, Washington, D. C.:

Telegram just received. Make cores four inches long, with six layers of number twenty wire; diameter an inch and one-half. Make outer coil half an inch wide and half an inch thick, of finest wire, and slip over inner coil at its end.

H. A. ROWLAND.

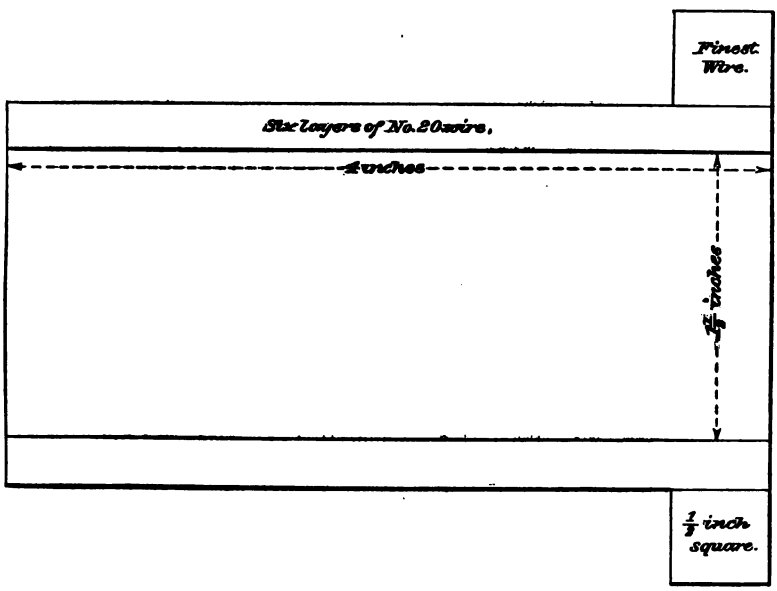
Letter following above telegram.

HUNTER, *July* 14, '81.

Professor NEWCOMB:

DEAR SIR: When your telegram arrived I had gone to the Hotel Kaaterskill, about ten miles from here, to stay over night. The telegram was sent back to the office to be sent to me, and my friends thought I had received it; but the stupid man here simply kept it and would not send it, because he had no authority to send it farther.

I have telegraphed back a reply. The dimensions should probably be about as follows:



Of course, the two must be precisely alike, and the induction coëfficients of one should be made to vary by a screw, which would move one of the secondary coils nearer to or farther from the end of the primary.

Yours truly,

HENRY A. ROWLAND.

P. S.—For these dimensions a telephone with many turns of fine wire in its coil would be best.

NOTE 8.— *Communications with Prof. D. E. Hughes, of London, England.*

(*a.*) *Cablegram to Mr. Preece, Superintendent Postal Telegraph, London, England.*

WASHINGTON, *July* 15, 1881.

To PREECE,
London:

Can Hughes suggest form of Induction Balance to locate leaden bullet in President? If so, cable at my expense.

GRAHAM BELL.

(*b.*) *Telegraphic reply to above.*

WHITNEY,
Washington, D. C.:

Preece says to Graham Bell, Washington: "Interesting and hopeful letter from Hughes with diagrams sent you to-day."

W. GREEN,
Pres. West. Union Telegraph Co.

(*c.*) *Note from Mr. Preece enclosing letter from Professor Hughes.*

GENERAL POST OFFICE, LONDON, ENGLAND, 19 *July*, 1881.

MY DEAR BELL: The enclosed very interesting letter from Hughes, will enable you to make some experiments, which I trust will result in success.

Yours, sincerely,

W. H. PREECE.

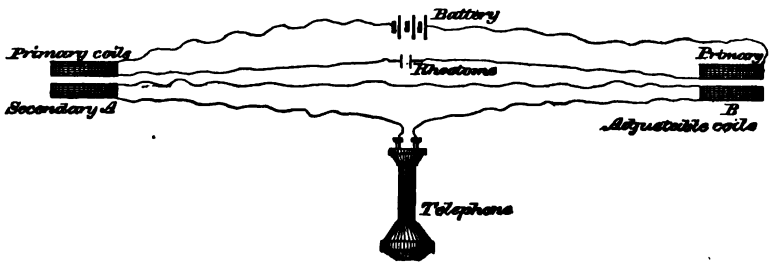
Prof. ALEXANDER GRAHAM BELL,
Washington, D. C.

(*d.*) *Professor Hughes to Mr. Preece.*

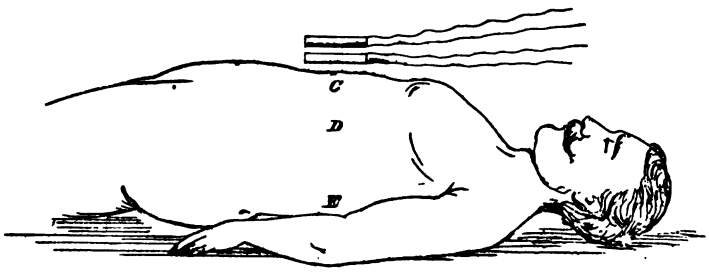
108 GT. PORTLAND STREET, W. LONDON, *July* 18, 1881.

DEAR PREECE: In reply to yours of to-day enclosing telegram from Prof. Graham Bell, I believe a specially-constructed Induction Balance could be made to locate the leaden bullet in President Garfield, provided the ball was nearer to one side of his body than the other.

Suppose we have two flat superposed coils on a single reel, so that these two coils form a single one as regards their relative distance; these coils, if connected with the usual adjusting coils of my balance, could be reduced to silence; then, on moving these coils near a metallic body, sounds would be heard. Thus the only difference would be instead of as usual taking the metallic body to the coils upon a fixed table, we take the coils to a fixed body in which we suppose metal, such as a bullet, to be hid.



Now, suppose we take coil A and move it over a body with a bullet, thus:



If bullet near C, we should hear it when coils just above it; at E, when the coils were below; but if bullet in the centre of the body, then we should have equal or probably no indications.

All this could be most easily done and with some results if the bullet had been of copper or silver, but lead has such a high resistance and gives, consequently, very feeble tones.

We can easily hear a copper penny at some inches distant from the coils, but lead requires that it should not be more than one or two inches distant: consequently, more sensitive coils would be required or a larger battery.

I think the experiment a hopeful one, and above all that the coils are easily made, and simple experiments could be first made to find a bullet in a mass of cotton, etc.

The microphone is invaluable as a probe for bullets. I made the first for Sir Henry Thomson to find out stone in bladder. But it is even more applicable to bullets. It consists of a simple hammer and anvil microphone

adjusted upon the handle of the ordinary probe. The instant this probe touches any hard substance a loud sharp click is heard in the telephone. The smallest shot can thus be heard, and there is a very distinctive tone between when the probe strikes a bone or bullet. This instrument is well known to surgeons, so I need not say more about it.

If you write to Professor Bell, please enclose this, as I should be pleased to hear of his success.

Sincerely yours,

D. E. HUGHES.

W. H. PREECE, F. R. S.

NOTE 9.— *Letter from Mr. Charles E. Buell to Col. Rockwell.*

Col. ROCKWELL:

DEAR SIR: Please suggest for me that in an electrical test to locate the bullet it will only be done with certainty by obtaining a balance, which can be done as follows: When the loudest tick is obtained by placing the inducing coil in the vicinity of the embedded bullet, then move another bullet of equal size towards the other coil of the bridge until a balance is had and no tick is audible, when the embedded bullet will be known to be a like distance from its respective coil.

CHARLES E. BUELL,
Electrical Engineer, New Haven, Conn.

NOTE 10.— *Note from Dr. Chichester A. Bell.*

N. Y. CENTRAL R. R., BETWEEN SYRACUSE AND BUFFALO,
July 16, 1881.

DEAR A. G. B.: If your coils are symmetrically arranged, should it not be possible to balance the effect of a bullet on one side by a bullet placed in a corresponding position on the other?

Probably you have thought of this before, but, if not, it may be worth a three-cent stamp.

Yours, truly,

CHICHESTER A. BELL.

NOTE 11.— *Note and telegram from Mr. J. H. C. Watts.*

BALTO., *July* 21, 1881.

Prof. A. GRAHAM BELL,
1221 *Conn. Ave., Washington, D. C.:*

Am working hard at our problem. Succeeded in reaching full three inches last night, but owing, I think, to my battery weakening somewhat, can hardly reach so far now. Won't you please advise me how you are getting along, and oblige,

Yours, very truly,

J. H. C. WATTS.

Telegram.

BALTIMORE, *July* 23, 1881.

Prof. A. G. BELL,
Washington, D. C.:

Have been unable to obtain satisfactory results since first trial and probably yours is at least as good as mine.

J. H. C. WATTS.

NOTE 12.— *Letter to Dr. Bliss.*

VOLTA LABORATORY, 1221 CONNECTICUT AVENUE,
WASHINGTON, D. C., *July* 23, 1881.

Dr. BLISS,
Executive Mansion:

MY DEAR SIR: You were kind enough to suggest that preliminary experiments should be made with the Induction Balance upon the person of Lieutenant Simpson, whose address you gave me. I accordingly communicated with the lieutenant, and he came to my laboratory last night in company with Dr. Stanton.

The new instruments we had hoped to use were not complete, so we had to content ourselves with the apparatus you tested the other day.

Upon passing the coils over the back of the lieutenant, it was found that at one spot a feeble sound made its appearance — too feeble, however, to be entirely satisfactory as evidence of the presence of a ball. I find that very feeble sounds like that heard are easily conjured up by imagination and expectancy, but the following facts seem to indicate that in this case the sound was due to an external cause — probably to the presence of a very deeply embedded bullet:

1. The sound, although it could not always be distinguished, uniformly appeared, when audible, *at the same spot.*

2. It was heard independently by Mr. Tainter, Dr. Stanton, and myself at the same spot. (If I recollect rightly, the lieutenant himself also located the sound, but my father could hear nothing.)

3. A blindfold test was then made. Mr. Tainter closed his eyes and turned away while the coils were moved over different parts of the lieutenant's back. The moment the sonorous spot was covered Mr. Tainter declared he could hear the sound. We are to repeat the experiments with our new instruments as soon as they are completed, and I shall report progress.

Yours, truly,

ALEXANDER GRAHAM BELL.

NOTE 13.—*Letter to Dr. Bliss.*

WASHINGTON, *July* 27, 1881.

Dr. BLISS:

MY DEAR SIR: Permit me to make a few remarks upon the value of the indications of the Induction Balance in determining the location of a lead bullet embedded in the human body.

Positive deductions concerning the location of the ball can only be safely made from positive indications of its presence. Negative indications may mean anything.

For instance: If we hear a sound from the telephone connected with the balance every time the exploring coil is passed over a certain part of the body, this may be taken as proof positive that a metallic mass is to be found in the neighborhood of the sonorous spot. If, on the other hand, we fail to obtain audible effects, we are not entitled to conclude that the bullet is absent from the part explored, or that it is embedded at a greater distance below the surface than the penetrating distance of the instrument used, for the bullet may be within any part of the area explored and close to the surface without affecting the balance if it is flattened, and if it is so located that the plane of its face is nearly perpendicular to the plane of the face of the exploring coil.

A few words may not be out of place here concerning the experiment made last night upon the person of the President.

Before entering the President's room, I applied the telephone to my ear, and heard a peculiar spluttering sound which it was found impossible to extinguish by any adjustment of the coils. I satisfied myself, however, that the arrangement was in a sensitive condition, by holding near it a flattened bullet. The hearing distance appeared to be about four cm. When the exploring coil was passed over the back of the President near the spine, no definite pulsation of the sound was heard; but when the coil was first passed over a portion of the front part of the abdomen near the right side, I heard at one point a sharp and sudden reinforcement of the sound. Upon moving the coil backwards and forwards a number of times over the same spot, I was unable to verify the observation. I should state hear that when a perfect balance is not obtained, it is not safe to place reliance upon a single observation of a pulsation in the sound, as a similar effect might be caused by an accidental irregularity in the vibration of the instrument used to interrupt the electrical current.

The sound heard was distinct and well marked, but it would not be safe to conclude that it was due to the presence of the bullet, unless the effect could be reproduced a number of times and always at the same spot.

I think I mentioned to you that it was discovered yesterday morning that the application of a tin foil "condenser" to the Induction Balance markedly influenced the hearing distance, increasing it in our experiments by about one centimetre.

A condenser was therefore connected to the arrangement used last night, but it has since been found that the condenser was only connected to one side of the balance, instead of to both. This mistake is enough to account for the difficulty experienced in adjusting the coils so as to obtain an acoustic balance, and for the observed reduction in the hearing distance.

If it is of importance to locate the bullet at once, I would recommend an immediate repetition of the experiment with the condenser properly arranged.

Whatever other results might be obtained, I feel sure we could, at all

events, settle immediately whether the sound I heard was due to the presence of the bullet or was an accidental phenomenon.

If it is not important to locate the bullet at the present time, it might be well to postpone a repetition of the experiment, so as to give Mr. Tainter and myself more time to improve our apparatus.

Yours, truly,
ALEXANDER GRAHAM BELL.

NOTE 14.—*Letter to Dr. Bliss.*

VOLTA LABORATORY, 1221 CONNECTICUT AVENUE,
WASHINGTON, D. C., *July* 31, 1881.

MY DEAR DR. BLISS: I write to let you know that my new form of Induction Balance gives brilliant promise of success. The indications with a flattened bullet are well marked and distinct at a distance of three inches, and audible effects can even be distinguished at five inches, but beyond three inches, silence and the greatest attention are requisite. Effects are produced at about two inches, when the bullet is held with its edge towards the instrument—a position that gave no results with our former apparatus.

Altogether I feel very much encouraged. The apparatus in its present form is a very clumsy affair, the surface that would be applied to the person of the President measuring seven inches by four. I hope to reduce the size of the apparatus very greatly in a day or two. In the meantime, should any necessity arise for an experiment upon the President, we have much better chances of success than at any previous time.

Yours, very truly,
ALEXANDER GRAHAM BELL.

NOTE 15.—*Letter to Dr. Bliss.*

VOLTA LABORATORY, 1221 CONNECTICUT AVENUE,
WASHINGTON, D. C., *July* 31, 1881.

Dr. BLISS,
Executive Mansion:

DEAR SIR: We have made experiments this evening upon the person of Private John McGill, an old soldier, who was wounded at the battle of Gaines' Mill, in 1862, and who still carries the ball that shot him.

I found no difficulty in finding a sonorous spot in his back, where undoubtedly the bullet lies embedded.

Mr. Tainter also located the bullet in the same place. Upon pressing with the fingers upon the spot a lump could be felt between two of the ribs. We experimented upon this same man yesterday, using a similar form of Induction Balance to that we tried upon the person of the President the other day, but could obtain no indications.

The new form of Induction Balance is so sensitive that a new difficulty is introduced by the effects produced by large metallic masses — for instance, gas lustres, iron fire-places, etc.

I think also that the earth's magnetism affects the result. We shall investigate these causes of disturbance to-night.

Yours, sincerely,
ALEXANDER GRAHAM BELL.

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16. *Report to the surgeons published in the daily papers Aug.* 2, 1881.

VOLTA LABORATORY, 1221 CONN. AVE.,
WASHINGTON, D. C., *Aug.* 1, 1881.

To the Surgeons in attendance upon President Garfield:

GENTLEMEN: I beg to submit for your information a brief statement of the results obtained with the new form of Induction Balance in the experiments made this morning for the purpose of locating the bullet in the person of the President. The instrument was tested for sensitiveness several times during the course of the experiments, and it was found to respond well to the presentation of a flattened bullet at a distance of about four inches from the coils.

When the exploring coils were passed over that part of the abdomen where a sonorous spot was observed in the experiments made on July 26, a feeble tone was perceived, but the effect was audible a considerable distance around this spot. The sounds were too feeble to be entirely satisfactory, as I had reason to expect from the extreme sensitiveness of the instrument a much more marked effect. In order to ascertain whether similar sounds might not be obtained in other localities I explored the whole right side and back below the point of entrance of the bullet, but no part gave indications of the presence of metal, except an area of about two inches in diameter, containing within it the spot previously found to be sonorous. The experiments were repeated by Mr. Tainter, who obtained exactly corresponding results. We are therefore justified in concluding that the ball is located within the above-named area.

In our preliminary experiments we found that a bullet like the one in question when in its normal shape produced no audible effect beyond a distance of two and a half inches, while the same bullet, flattened and presented with its face parallel to the plane of the coils, gave indications up to a distance of five inches. The same flattened bullet, held with its face perpendicular to the plane of the coils, produced no sound beyond a distance of one inch. The facts show that, in ignorance of the actual shape and mode of presentation of the bullet to the exploring instrument, the depth at which the bullet lies beneath the surface cannot be determined from our experiments.

I am, gentlemen, yours truly,
ALEXANDER GRAHAM BELL.

On Secondary Batteries. By George F. Barker, of Philadelphia, Pa.

[ABSTRACT.]

A SECONDARY battery is one which produces a current in consequence of a previous electrolytic condensation or polarization upon the surfaces of its plates. Electrolytic polarization and the production of a secondary current by means of it, are almost as old as the voltaic battery itself. In 1801, Gautherot, having used platinum or silver wires as electrodes in a solution of salt, noticed a momentary inverse current when the battery was replaced by a galvanometer. Ritter observed the same phenomenon in 1803 with gold wires. He constructed a secondary battery with pieces of gold separated by cloth disks moistened with salt water. After contact for some seconds with a Volta's pile of more pairs than itself, it produced a current to which Ritter gave the name of secondary current. Platinum, gold and silver, gave a more marked result than copper, brass, iron or bismuth; and no effect at all was obtained from lead, tin or zinc. Fifty disks of copper separated by disks of cloth moistened with salt or sal ammoniac solution, when charged with a Volta's pile of 100 pairs, gave a current which decomposed water and produced all the effects of the primary current. Volta himself, as well as Marianini, explained the results by supposing that the decomposition of the salt produced acidic and basic deposits upon the metal disks; a view which Becquerel apparently confirmed by immersing a platinum plate in an acid and another in an alkali and noting that the current was the same in direction as the secondary current. But in 1826 De la Rive obtained the secondary effect from platinum plates immersed in dilute sulphuric acid; and since no basic or acidic deposits were now possible, he regarded the phenomenon as a physical one due to a special or polarized condition of the plates themselves. The current he called a polarization current. In 1833–4, Faraday's researches on electro-chemical decomposition definitely established the proportionality of the current to the amount of electrolyte decomposed. He observed the "peculiar state" of the plates in his voltameter; and even noticed that when lead acetate was electrolyzed, metallic lead was deposited on the negative plate and lead peroxide on the positive; but he offered no explanation of these phenomena. In 1842, Grove, following

out an observation of Matteucci, constructed a gas battery in which the current was produced from two plates of platinum immersed in oxygen and hydrogen gases respectively. In 1852, C. W. Siemens used two carbon plates in lead acetate solution as a secondary battery; obtaining, by the action of the current, lead on one of these plates and lead peroxide on the other. One cell of this battery decomposed water; so that the electromotive force of the current must have been nearly two volts.

In 1859, Planté began an elaborate research upon this secondary action, studying carefully the influence of the metal and of the solution employed. The maximum effect was given by lead plates immersed in dilute sulphuric acid; and in 1860, he produced secondary batteries of remarkable power by rolling two sheets of lead together, a strip of insulating material being between them, and placing them in a ten per cent sulphuric acid. When connected with a battery, the oxygen evolved upon the positive plate attacked it and converted its surface into peroxide; the hydrogen for the most part escaping. When now the current was reversed, the hydrogen reduced this peroxide to finely divided metallic lead and the oxygen attacked the other plate, producing peroxide. By such alternations, the metal may finally be corroded to any depth and the amount of finely divided lead and peroxide indefinitely increased. In Planté's experiments this forming process required three months. In 1881, Faure brought out a secondary battery in which, as in Planté's, lead plates in sulphuric acid were employed. But in order to avoid the tedious process of "forming," in which the deposits upon the plates were produced by electric action, Faure covered the plates at the outset with a layer of minium or red lead. Upon passing the current, this red lead was reduced to metallic lead on the one plate and oxidized to peroxide on the other; thus producing the battery charged and ready for use in a single operation requiring but a few hours.

The theory of the storage of energy by secondary batteries is not difficult. Energy is capacity of doing work and work is force acting through distance. Whenever two attracting bodies are separated work is done upon the system and energy is absorbed by it; and this whether the attraction overcome is mechanical, molecular or atomic. When an electrolyte is decomposed by an electric current, the current yields energy in proportion to the amount of the ions separated. The quantity of lead peroxide

formed in a secondary battery represents the amount of energy stored from the current. So soon, however, as the separating force ceases to act, the ions reunite and the stored energy becomes again free. Since the amount of energy thus set free, like that originally stored up, is proportional to the quantity of matter concerned, it is clear that the amount of electricity obtained from a given secondary battery is proportional to the amount of electrolytic products deposited upon its plates. In theory, the electrical energy set free should equal that originally stored up; but in practice this result is never attained.

In its primary expression the chemistry of the secondary battery is simple. In the form made by Grove, two platinum plates are placed in acidulated water. Under the action of the current, oxygen is set free on one of these and hydrogen on the other,[1] and the plates are said to be polarized. The electromotive force between these plates is the sum of the contact-potentials involved. Its value is 1.45 volts and it measures the chemical attraction between oxygen and hydrogen. If now the two plates be connected by a wire, the oxygen and the hydrogen recombine and the energy of combination appears in the form of a secondary current. In general, however, the electrolytic action is accompanied by secondary chemical changes which render the phenomenon much more complicated. In the lead battery, for example, secondary products appear at both electrodes. The oxygen set free at the anode combines with the lead, the final product there being peroxide. The hydrogen evolved at the cathode reduces the oxide which it finds there and produces metallic lead. As before, the electromotive force in the battery is the sum of the contact-potentials; and this, in the case of lead and lead peroxide, is a little above two volts.[2] When now the plates are connected by a conductor, the entire process is reversed. Water is decomposed, and, since the immersed portion of the plates changes sign on discharge, the hydrogen now goes to the peroxide surface and oxygen to the finely divided metallic lead. The former is deoxidized to monoxide and the latter oxidized to the same condition. Simultaneously, a current flows through the

[1] Undoubtedly the oxygen is produced by a secondary reaction.

[2] Planté observes that, after complete charging, the electromotive force may rise above two volts at the instant of removing the battery. But as it falls to this value at once on closing the circuit and remains so, he attributes the first result to the temporary production of a peroxidized substance, as hydrogen peroxide or persulphuric acid, at the anode.

conductor, the electromotive force of which is two volts, the strength of which is the ratio of this electromotive force to the resistances in circuit, and the duration of which depends upon the amount of secondary products deposited upon the plates during the charging. Beside this, however, another action takes place in the secondary battery which is of great importance. The sulphuric acid in which the plates are immersed attacks both the peroxide on the one plate and the metallic lead on the other and converts both into lead sulphate. Moreover, as fast as the monoxide is produced by the discharge of the battery, it also is converted into sulphate. So that upon complete discharge, the plates are precisely alike, both being covered with lead sulphate.

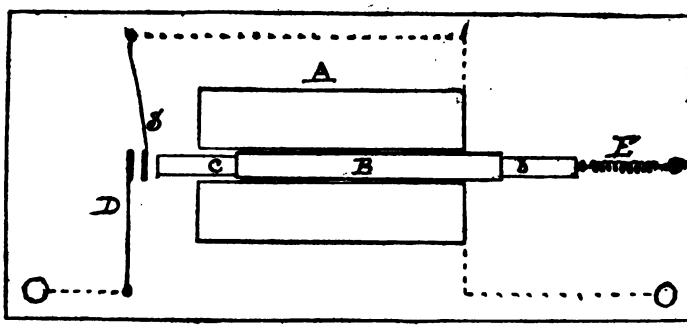
The secondary batteries employed for experiment were those of Planté and Faure. Of the former type, two cells of the larger size made by Breguet were used. Of the latter, one cell of medium size made by Reynier, and thirty-four cells of the size exhibited at the Electrical Exhibition at Paris, were experimented with. The battery of thirty-four cells was made in January under my direction.[3] Each cell consisted of six plates, each 20 by 60 centimetres, the lead being a millimetre thick. The exposed surface of each plate, therefore, was nearly 0.25 of a square metre, and of each cell about 1.50 square metres. For the whole battery the surface was about 50 square metres (exactly 48.96). Each plate was covered on both sides with a coating of red lead about a millimetre thick made into a paste with water. Parchment paper was then folded over the whole, and over this a covering of felt sewed together at the edges. The plates were then folded transversely and each pair intercalated. Heavy lead terminals were attached to each plate, and three pairs of plates were placed in each box, the terminals of one plate of each pair being united together and furnished with a connector. The boxes were lead-lined and measured 31 cm. in length, 25 in depth and 11 in width. One of the two sets of plates was in contact with the lead lining, the other was insulated. The latter was always made the positive end. The total weight of a single cell was 17 kilograms.[4]

[3] This battery, as well as the cut-out, Fig. 1, was made by Jas. W. Queen & Co., of Philadelphia.

[4] Mr. W. Lachlan, the electrical engineer of the "Light and Force Company," of New York, has kindly furnished me the following figures concerning the Faure batteries of improved construction recently imported in the steamer Labrador. Four types of Faure batteries are made in Paris, called A, B, C and D. Type A consists of seven positive plates, each 37 × 23.5 cm. and 3 mm. thick, perforated with holes 6 mm. in diameter 10 mm. apart, alternated with seven negative plates of the same size, but

The charging current was generated by a Gramme machine whose field was maintained by an auxiliary dynamo. The maximum obtainable electromotive force of this current was 85 volts. In general, however, a much less electromotive force was used, sufficient only to give a current of from 10 to 20 amperes through the thirty-four cells arranged in series. Currents as high as 45 amperes, however, were occasionally employed. At the outset, considerable difficulty was experienced from the discharge of the battery when from any accidental cause the current fell below the potential of the thirty-four cells. The wear of the brushes of the auxiliary dynamo sometimes opened its circuit and thus destroyed the magnetic field of the Gramme. There being now no counter elec-

FIG. 1.



tromotive force to oppose it, the secondary battery current flowed back through the machine, discharging the battery completely. A simple device was arranged to prevent loss of the charge in this way should the charging current become weakened. This device is represented in fig. 1, where A is a coil of No. 10 copper

only 0.5 mm. thick, and similarly perforated. The seven positive plates weigh 16.66 kilograms; the seven negative ones 2.77 kilograms. The red lead spread on their surfaces weighs 22.13 kilograms, the two exterior plates being covered only on their interior surfaces, so that there are twenty-six active surfaces in all. Each plate has an electrode 7 cm. wide. The box is 45 × 20 × 40 cm. in size, is pitched throughout, and weighs with its cover, 10 kilograms. The battery complete, ready for charging, weighs 63 kilograms. In type C there are five positive plates each 24 × 19 cm., 3 mm. thick, and four negative plates 0.5 mm. thick, both perforated with holes 5 mm. in diameter, and a centimetre apart. The positive plates weigh 7.14 kilos, the negative 0.898 kilos. Sixteen surfaces are covered with red lead, weighing 8.28 kilos. The electrodes are 3.5 cm. wide. The box weighs 3 kilos., and the battery complete 20.37 kilos. Each cell of type A contains, it is claimed, when fully charged, 375 ampere-hours of current delivered at an electromotive force of 2 volts. Hence it will furnish, for example, a current of 25 amperes for fifteen hours. Type C contains 150 ampere hours. Types B and D are round cells in jars of earthenware. The former weighs 35 kilos., and contains 225 ampere-hours; the latter 8 kilos. and contains 75.

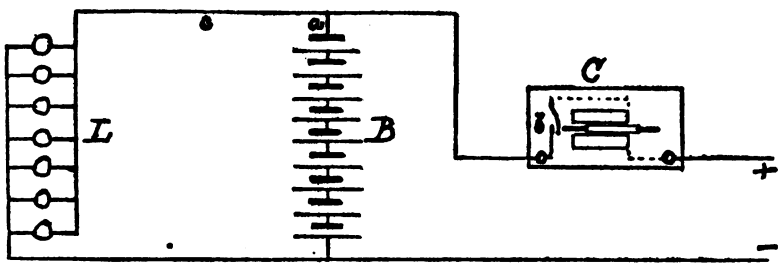
wire. Within its core is a rod of iron B, prolonged at both ends by brass rods of smaller diameter, *b* and *c*, these being supported upon rollers. Opposite the end of one of these rods is a flat spring S, faced with platinum and making contact when depressed with a similar brass strip D, also faced with platinum. The course of the current is shown by the dotted lines. When the circuit is closed between S and D, the current traversing the coil draws the iron bar strongly into its axis, and the end of the brass rod pressing against S holds the circuit closed. Should the current weaken, the spring S reacts against the rod pushing it back and opening the circuit. By means of an auxiliary spiral spring E, the instrument may be adjusted to any required current-strength. It would seem that by no possibility can the accumulated charge in the battery be lost, when this automatic cut-out is in circuit. As now described, the coil is of large wire, and the entire current passes through it. But a second form of it has been used in which the coil is made of finer wire and is placed in a shunt circuit as shown in fig. 3. In both cases of course, the entire current passes through the contact springs.[5]

After charging, the current from the secondary battery was used to maintain seventeen Edison incandescent lamps of the B pattern. More frequently, however, the battery was used in connection with the Gramme machine. The source of power being an Otto gas engine which, at best, has an explosion only every fourth stroke, the motion appears very irregular when tested by so delicate a method. As every explosion increases the speed, the higher electromotive force developed increases for an instant the brightness of the carbon filament, producing a marked pulsation in the light. If now a secondary battery of nearly the same electromotive force be placed in multiple circuit with the machine and lamps, it acts as an equalizer, and destroys entirely the pulsations. Figs. 2 and 3 show the arrangement actually employed, in which B is the secondary battery, and L the lamps; the automatic cut-out being at C. When the circuit is opened at *a*, *b* and *c* being closed, the lamps are fed by the machine alone; when opened at *b*, *a* and *c* being closed, by the battery alone; when at *c*, *a* and *b* being closed, the machine charges the battery. When all three are closed, the lamps are fed by the machine and bat-

[5] Sir Wm. Thomson has devised a polarized cut-out for this same purpose. He exhibited it at the York meeting of the British Association in September last.

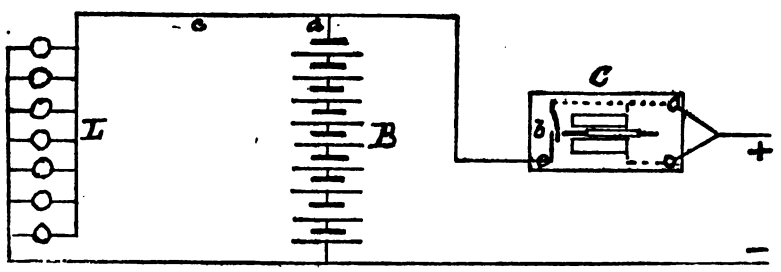
tery together, the latter acting as an equalizer. A galvanometer inserted at *a*, *c* being opened and *b* closed, indicates the charging current; when *c* is closed and *b* open, the discharge current; and when *b* and *c* are both closed, the equalizing current. If the electromotive force of the machine-current be but slightly in excess of the battery current, the deflection to the right in the

FIG. 2.



first case will about equal that to the left in the second; so that in the third case the deflection is practically zero, not the least spark appearing on opening at *a*; and this, though the pulsations are completely destroyed, the light being perfectly steady. The

FIG. 3.



advantage of a much more agreeable light is thus secured; but in addition the life of the lamps must be increased by the use of this steadier current, as it has been shown to be by using the secondary battery alone.

On the 27th of February the battery was charged continuously for fourteen hours, or until hydrogen was freely evolved from each of the thirty-two cells employed. It then gave an electromotive force of 59.3 Daniell cells; or calling the Daniell 1.08 volts, of

64.04 volts. On the tenth of June, the same thirty-two cells gave an electromotive force of 56.7 Daniell's, or 61.2 volts. On the 24th of June, their electromotive force was 54.3 Daniells', or 58.6 volts. It may be assumed then that the average electromotive force of a single cell was not far from two volts. This value has never been exceeded in any of the measurements made, either on single cells or on groups of cells. When first made, the resistance of the single cells was low, about 0.02 ohm; but subsequently the internal resistance increased considerably, rising even to twice this value.

The chief defect of the secondary lead battery is the local action which takes place in it. By local action is meant any deterioration in the efficiency of a battery which occurs when it is left on open circuit. This defect was noticed by Planté, who says: "If the lead peroxide deposited on the positive plate had no tendency to spontaneous reduction in the acidulated water, by forming a local circuit with the metal that it covers, the preservation of the charge taken by the secondary couple should be indefinite. But this peroxide is reduced, and with the more facility as the layer of it is thinner. In a new secondary couple the charge cannot be preserved." In a well formed battery he attributes the preservation of the charge to a reduction of the superficial layer to monoxide, which acts as a protecting coating. Another local action, however, takes place in this battery which appears to be of more importance. This is the action of the sulphuric acid upon the products of electrolysis. The finely divided lead on the negative plate, as well as the peroxide upon the positive, is alike converted into sulphate by this action. When the circuit is closed this action is normal; but it takes place equally well when the circuit is open. It is then abnormal and of course injurious. The formation of lead sulphate in the secondary battery has been studied by Gladstone and Tribe.[6] They found that with a Planté cell the whole of the peroxide formed on charging was converted into white sulphate within seventeen hours. The same fact was observed with the Faure battery. So far however, from regarding this formation of sulphate as wholly injurious, they regard it as "absolutely requisite in order that the charge should be retained for a sufficient time to be practically available." They believe that the insoluble lead sulphate clogs up the interstices of the

[6] Nature, XXV, 221, 461; XXVI, 251, 1882.

peroxide and after a while forms an almost impermeable (*sic*) coating between it and the lead plate; so that the mass of peroxide which is in contact with the lead plate expends its energy slowly, while the same mass of peroxide, brought into connection through its own lead plate with another lead plate at a distance, expends its energy even through the sulphuric acid, in a tenth or a hundredth part of the time.

My experiments with the Faure battery above described confirm entirely those of Gladstone and Tribe as to the formation of lead sulphate. In several cases, the acid in charged cells disappeared entirely on standing so that the water was tasteless; and in some instances this was repeated three times with the same set of plates. This action increased very greatly the internal resistance, first by replacing the acid by water and second by forming the higher resistant sulphate. On examining the plates, lead sulphate formed the entire coating upon both of them. On attempting now to recharge such a cell, this notable increase in the resistance showed itself in the reduced current transmitted by the same electromotive force. Gradually, however, the resistance fell and the current finally regained its normal strength. In one instance, the charging current rose from 2 amperes to 12, in four hours, 28 cells being in circuit, arranged in series. It was observed, however, that when the current was first applied, torrents of gas were evolved from both plates; and that as the charging process went on the oxygen was first completely absorbed and then the hydrogen. When the charge was nearly complete the evolution of hydrogen reappeared. There would seem, therefore, to be a very considerable loss of energy, due to the resistance consequent upon the formation of sulphate. In the first place the current energy is wasted as heat in the cells; and in the second, a large portion of it is dissipated in the evolved gases.

Another defect of considerable magnitude is developed when a number of secondary cells is placed in series, and is due to a want of uniformity in their capacity for storage. This arises probably first, from the difficulty of constructing them exactly alike originally, and second, from the impossibility of uniform action among them on charge and discharge. The difference in the cells of the same battery even, when they all have been treated precisely alike, is surprising. On the 14th of June, thirty-two cells were freshly charged, and the current strength from each cell measured on a tangent galvanometer. Eleven cells gave 80° deflection each,

seventeen gave 79° each, one gave 78°, one 77°, one 75°, and one 20°. When seventeen Edison B lamps were placed in circuit, the discharge deflection of the thirty-two cells in series was 67°. On the following day the battery was charged again, and, after an hour's use on the lamp circuit, was again tested. Those giving a deflection of 87° were two; of 86°, two; of 84°, two; of 83°, one; of 81°, one; of 78°, five; of 77°, two; of 76°, one; of 13°, three; of 11°, one; of 10°, three; of 8°, one; of 6°, one; of 5°, three; of 4°, one; of 2°, one; of 1°, one; and of 0°, one. Obviously, so long as any peroxide is present, the electromotive force is constant; hence this wide variation of current strength must be attributed apparently to the variation of the resistance in the different cells. But this is not the worst aspect of the matter. If the discharge be continued for a longer time, some of the cells become exhausted sooner than others, and are then charged in the inverse direction by the current from the rest. On the 12th of June, the thirty-two cells, after pretty complete discharge, were measured as above. Nineteen cells gave positive deflections ranging from 87° to 5°; thirteen gave negative deflections varying from 18° to 1°. Faraday said of primary batteries in 1834, that "weak and exhausted charges should never be used at the same time with strong and fresh ones in the different cells;" and this remark is even more applicable to secondary batteries, where this condition is more liable to occur. Not only does a dead cell introduce resistance into the circuit, but what is of more importance, its reverse polarization introduces a counter-electromotive force, so that every such cell neutralizes another cell, the dead resistance of which is also added to the circuit. In consequence, it is not safe in practice to draw from a secondary battery all the energy it should be capable of yielding, and in most cases not more than half can be safely taken out of it. In place of continuing uniform as in a single cell, the electromotive force of a series of cells begins to fall when about half the charge which it ought to be capable of yielding has been drawn from it. The practical result is that to supply for lighting a definite number of coulombs of electricity, a secondary battery of twice the size is necessary under these circumstances.

With reference to the two forms of lead secondary battery devised by Planté and by Faure respectively, my experiments show that while the electromotive force is the same in both, and while the internal resistance and storage capacity may readily be made the same, yet that the local action is far less when the peroxide is

formed by the action of the current upon the lead plate itself than when it is produced from minium spread upon it. The rapidity of the formation of the peroxide is proportional to the density of the current; but when rapidly formed it is fine and spongy. In one of my Planté cells, the peroxide is beautifully crystalline and very hard. Not a trace of sulphate has been formed in it apparently, though it has been in use for six months, and has been frequently charged and discharged during that time. The necessarily finer condition of the peroxide in the Faure battery would seem to predispose it to a more considerable local action. True, the Faure battery requires very much less time than the Planté to form it. But this is more than compensated by the increased permanence of the latter. With an equal storage capacity, as in M. de Kabath's form of Planté cell, it would appear that in efficiency it will surpass the form devised by Faure.

The commercial aspect of the question of the storage of electrical energy has recently assumed a very considerable importance. But it is evident at the outset, that an agent, which has been produced and then stored, must cost more when redelivered than when first produced, by exactly the cost of the storage; that is, supposing there is no loss in storing. But there is a loss, and this is almost if not quite one-half of the energy involved; as it would seem from the Conservatoire experiments in Paris, among others. The first cost of the accumulators, the expense of charging them, their low efficiency, and especially their bulk and weight, must ever prevent, it would seem, their competing successfully with the direct use of the dynamo-electric machine, at least with any form of secondary battery yet devised. For special uses, however, the storage battery has a high value. Even if the cost of an ampere of current produced by it is twice as great as if produced directly by the dynamo-machine, this cost is only one-half of that required to give the same current by any available form of primary battery. Where a strong current of low electromotive force is needed, under conditions where a dynamo cannot be employed, there the secondary battery has its most important field. Its value rapidly decreases as the number of cells is multiplied, as when a high electromotive force is needed for the production of light. Its chief advantage is its transportability; since by placing it near the work to be done, the loss of energy on long conductors is obviated.

On modifications of the Spectrum of Sodium vapor in a Bunsen Flame. By Charles S. Hastings, of Baltimore, Md.

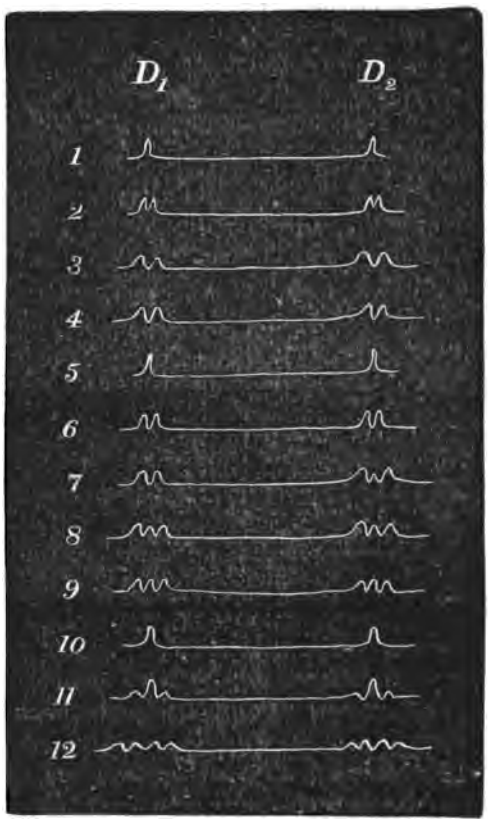
[ABSTRACT.]

At the Boston meeting of the Association, Prof. Young described an experiment, with a sodium-tinted Bunsen flame having a lime-light behind it, which in some respects appeared to offer contradictions to the fundamental theory of spectrum analysis. To an explanation of the phenomenon in accordance with accepted theory which I suggested in a letter, he objected that it implied the existence of phases which he had not observed. To satisfy my own mind I tried a series of experiments shortly after, which presented some striking results, predicted, it is true, by the explanation which I had entertained, but worth noting.

In front of the slit of the large spectroscope belonging to the Johns Hopkins University, was placed a Bunsen burner with its top about two inches below the slit. This gave a flame seven inches in length, hollow for more than half its length and with well defined inner and outer walls. The dispersing member of the spectroscope was a Rutherford grating of over 30,000 lines, and the observations which follow were made in the spectrum of the second order with a magnifying power of 26. This power was quite sufficient to show the duplicity of b_3 and b_4, and even of E_1. Into the flame, a quarter of an inch below the level of the slit, was introduced a bead of fused chloride of sodium; a less volatile compound would not yield the results here described.

When the bead was gradually moved through the flame, starting from the side next the slit, the spectrum underwent a series of changes of which the typical forms are given in the cut from 1 to 10 inclusive. Here the spectral lines are represented by the ordinary convention, abscissas representing deviations and ordinates the corresponding intensities. Thus 1, 5, 10 corresponding to positions where the bead was hardly touched by the flame—5 being in the centre of the hollow portion—represent simply two sharp bright lines; 2, 3, 4, 6 indicate lines more or less widened with a narrower dark central line; 8 and 9 exhibit the lines still wider, so that they may be more properly called bands, striped by two black lines each; 7 is interesting as emphasizing a difference in the two sodium lines which are ordinarily so like. By lowering

the flame until the top of the hollow centre was but just above the slit, a spectrum represented by 11, which may be regarded as a simple modification of 9, was obtained; and if, while in this position, another bead was introduced cautiously into the *side* of the flame and lower down, the phase described by 12 might be observed. This form of the sodium spectrum at low temperature, consisting of eight bright lines defining six dark ones, I have never had any difficulty in reproducing when the pressure on the gas mains was sufficient to give a good flame.



The explanation of these phenomena, though in some cases a little involved, is easy to one who has mastered the fundamental principles of spectroscopy.

Perhaps it can be given in the fewest words by formulating certain laws governing gaseous radiation, viz.:

(*a*) A gas can only produce a dark line by absorption when at a lower temperature than the source of radiation.

(*b*) Even then it can produce a dark line only when it exists in a sufficient abundance.

(*c*) The brightness of a spectral line can be increased in two ways, either by increasing the number of molecules which send the proper vibrations to the objective of the collimator, or by

increasing the temperature of the gas of which the molecules are a part.

(*d*) By increasing the density of a gas, or by increasing its temperature at a constant density (in other words, by increasing the frequency of molecular impact), its spectral lines become broader.

These laws may be applied as follows: When the bead touches the flame at any one point of its four surfaces in line of axis of collimator, only a small amount of sodium is volatilized, and this rare hot vapor gives well defined lines. If the bead is gradually carried into the flame from the hither side the vaporization first goes on more rapidly and the denser gas yields widened lines, until, when the bead is fully enveloped by the flame, the copious evolution of sodium vapor causes it to overflow the limits of the flame and form a layer of cooler vapor between the hot flame and slit. This gives rise to the forms 2, 3, 4, according as the density varies in different portions. The form 6 corresponds to the case when the sodium bead enters the farther surface of the hollow cone within the flame. This, as is well known, is the hottest portion of the flame, though in this case its temperature is lowered over the solid chloride of sodium, the evaporation of the salt contributing to this end more powerfully perhaps than conduction; thus the physical conditions are not essentially different from those which obtained on the nearer side of the hollow cone and 6 should resemble 3 and 4. As soon as the solid is moved beyond this position, however, the conditions are greatly changed, for then there is a layer of gas at an extremely high temperature between the slit and bead, and very near the latter. If this layer is impregnated with a small amount of sodium vapor, we shall have, according to the laws which have been formulated, bright sharp lines superimposed upon the spectrum of the colder vapors beyond. These last, however, may give such a spectrum as 3, whence the forms 7, 8, 9. If the relative importance of the hot layer be increased by lowering the flame until the conical summit of the inner mantle is nearly opposite the slit, form 11 is easily reproduced. Finally, if, with this spectrum in the field of the spectroscope, we introduce a layer of rare and cool vapor between its source and the slit by means of a second bead, the last form can be secured with little difficulty.

SIEMENS' ELECTRICAL DEEP-SEA THERMOMETER. By J. R. BARTLETT, Commander U. S. Navy.

[ABSTRACT.]

THE principle of this new apparatus for obtaining temperatures of sea-water below the surface is based upon the variations in resistance of metals to the passage of an electric current arising from changes of temperature.[1]

The apparatus was constructed by Mr. Siemens of London, for the U. S. Coast Survey and placed on board the steamer "Blake." It consists of 2600 fathoms of double insulated cable, which is wound on a large reel on the upper deck. To the end of this cable is attached a resistance coil, which can be lowered to any depth desired.

The other ends of the insulated cable are brought to a room on the main deck and are attached to a Wheatstone bridge specially arranged for this purpose. Another resistance-coil of the same material and resistance as that lowered into the sea is likewise attached to the bridge. The latter coil has an attached thermometer and is kept in a copper vessel filled with water. A battery of twelve Leclanche's cells and a Thompson's marine galvanometer are attached to the bridge. The resistance-coil at the end of the cable being lowered into the sea just below the surface and some of the same water placed in the copper vessel containing the comparison-coil, the reflected pencil of light, when a current is sent into the bridge, will be at zero on the scale, as under these conditions both resistance-coils are in the same temperature. If the cable is then paid out and the resistance-coil at its end reaches any depth, say 100 fathoms, where the temperature is lower, the pencil of light will be deflected to the left, when the circuit is closed. Now, if cold water is added to that in the copper vessel containing the comparison-coil, with the circuit closed, until the pencil of light comes back to zero on the scale, it is assumed that the water around each coil is the same, and the reading of the attached thermometer of the comparison-coil will give the temperature at 100 fathoms, the depth to which the cable has been lowered. After taking a series of temperatures to 800 fathoms in this way, the cable was reeled in, stopping at intervals for temperatures; in this case, with circuit closed, the deflection of light was to the right, and it was

[1] A diagram, showing arrangement of apparatus, is given in Jour. of American Geographical Society, Vol. XIV, 1882; also in No. 20 of the Proceedings U. S. Naval Institute.

necessary to raise the temperature in the copper vessel until the light was brought to zero on the scale.

Water frozen by a Carré ice machine was used to reduce the temperature of the deck resistance coil.

Trials were made in latitude 36° 50′ N., longitude 74° 25′ W., during a perfect calm, with smooth sea and the ship's engine at rest, under which conditions the results of our several lowerings were very satisfactory. Having taken many thousand deep-sea temperatures with the Miller-Casella thermometers, my confidence in them was greatly strengthened when, after repeated lowerings, their readings agreed with the Siemens apparatus. Readings can be taken with the latter to one-quarter of a degree; but with the Miller-Casella it is difficult to decide on half a degree.

In a previous trial in the strength of the Gulf Stream, off Charleston, during moderate weather, with the vessel rolling 15°, it was found very difficult to read the galvanometer, and this could only be done when the engine was at rest. Under these trying circumstances, however, the temperature agreed with the Miller-Casella. I give an example of a single lowering off the Chesapeake.

	Depth in Fathoms.												
	Surf.	5	10	15	20	30	50	75	100	150	200	300	400
Siemens App..	76¼	76¼	76¼	69	58	54	54½	52½	50½	46½	43½	40½	40
Miller-Casella..	76¼	76¼	76	68	58	54	53½	52½	50½	46½	43½	40½	40

The above was taken on August 11; on the 12th the Siemens thermometer indicated a warm stratum of water between thirty and seventy-five fathoms; the Miller-Casella gave only the maximum and minimum, and did not indicate this. The temperature at twenty fathoms was 57° with both thermometers, at thirty, 52°; but at fifty fathoms the Siemens thermometer read 54¼°, while the minimum of the Miller-Casella read the same as at thirty fathoms; at seventy-five fathoms the Siemens read 53°, the Miller-Casella still 52°; at 100 fathoms they agreed at 50½°. These observations were continued during the entire day with always the same results.

By lowering the Miller-Casella thermometer at the rate of 200 fathoms per minute to 50 fathoms, and hauling it by the 30 fathom depth at the same rate of speed, the temperature would read as high as 53½°.

AN INSTRUMENT FOR READILY PRODUCING LOW TEMPERATURES. By HENRY CARMICHAEL, of Brunswick, Me.

[ABSTRACT.]

THE instrument exhibited consists substantially of three test tubes placed within one another.

The outer tube is four centimetres wide and thirty-one centimetres long. The middle tube is of the same length and two and a half centimetres wide. Upon it have been blown a series of enlargements for increasing the surface, which, however, do not prevent a free passage of air between the tubes. The bottom of the latter tube contains numerous perforations which have been made by plunging a red-hot needle through the glass already softened before a blast lamp. This tube is held in place by a rubber ring which makes a tight joint at the top, and by a suitable aperture admits an air current to the annular space between the tubes. The bottoms of the tubes are one-half a centimetre apart.

The third tube is somewhat longer than the others and one centimetre wide. It is intended to hold the substance to be submitted to a low temperature.

For producing low temperatures, liquids are used which in a current of air blown through them are rapidly vaporized.

The operation of the instrument is as follows: 50 c c. of the volatile liquid is introduced in the middle tube. The aperture in the rubber ring is connected by flexible tubing with bellows, and the air-current thus introduced at the top of the narrow annular space between the outer and middle tubes flows to the bottom and there passing through the fine perforations beats the liquid into foam.

If ether or the petroleum distillate "gasolene" is used, the liquid is quickly reduced to —23°C. Six grammes of water placed in the inner tube is speedily frozen.

In conducting this experiment to save the tube, it is important to withdraw it before the water is congealed to the centre. The ice cylinder is easily shaken out of the tube after warming it in the hand.

By substituting more volatile liquids in the evaporator, lower temperatures are reached. Seventy-five grammes of mercury are almost instantly solidified in a bath of sulphurous anhydride.

When the vapors discharged are undesirable in the work-room,

they are conveyed away through flexible tubing connected with an aperture in a cork ring which forms there a joint between the middle and inner tubes.

The effectiveness of this apparatus depends largely upon the fact that the escaping cold saturated current is made to cool the warm entering air before it comes in contact with the liquid to be evaporated.

Whatever moisture is contained in the air is deposited as frost upon the middle tube, and when the transparency of the latter is to be preserved the air is previously dried.

The instrument described is not merely for illustrating the fact that evaporation is accompanied by an absorption of heat, but is intended as a convenient means of securing for a short or long period the low temperatures necessary in many physical and chemical researches.

By a slight change in its construction, the perforations are placed above the bottom, and the region of intense cold remains free from the foam that might otherwise interfere with the direct observation of phenomena at the bottom of the middle tube.

UPON A PROPOSED METHOD OF PRODUCING ARTIFICIAL RESPIRATION BY MEANS OF A VACUUM JACKET. By ALEXANDER GRAHAM BELL, of Washington, D. C.

[ABSTRACT.]

I PROPOSE to surround the waist of the unconscious patient by a rigid jacket or drum somewhat larger in diameter than his body. The apparatus can be rendered practically air-tight by a rubber band round the thorax, and another round the loins. Upon exhausting the air inside the drum a partial vacuum is produced around the abdomen. Under such circumstances the pressure of the atmosphere forces air through the mouth and nose into the thorax causing the depression of the diaphragm and consequent expansion of the abdomen.

The alternate rarefaction and condensation of the air confined around the abdomen thus cause alternate inspiration and expiration.

RADIANT HEAT AN EXCEPTION TO THE SECOND LAW OF THERMODYNAMICS. By H. T. EDDY, of Cincinnati, O.

[ABSTRACT.]

THIS paper attempts to show that the second law of thermodynamics must receive some modification in the case of radiant heat, by reason of the fact that heat is not propagated through space instantaneously.

TITLES OF OTHER PAPERS READ IN SECTION B.

DANGERS FROM LIGHTNING INCREASED BY TELEGRAPH WIRES. By Stephen S. Haight, of West Farms, N. J.

ATOMIC PHYLLOTAXY. By Pliny Earl Chase, of Philadelphia, Pa.

THE TENSION OF THE LUMINIFEROUS ETHER. By De Volson Wood.

THE FORCE OF DIFFUSION OF THE GASES FORMING THE ATMOSPHERE. By George Iles, of Montreal, Canada.

*THE COLOR OF THE SUN. By S. P. Langley, of Alleghany, Pa.

*ELECTRIC INDUCTION BY STRESS. By S. W. Robinson, of Columbus, Ohio.

*A NEW SOIL-THERMOMETER. By D. P. Penhallow, of Mountainville, N. Y.

*ON TEMPERATURE OF THE SUN. By A. E. Dolbear, of College Hill, Mass.

ON THE TELEPHONE AS AN EXPLORER IN AN ELECTRIC FIELD. By A. E. Dolbear, of College Hill, Mass.

*ON THE CHARACTER OF HEAT. By A. E. Dolbear, of College Hill, Mass.

*The papers thus marked were only read by title owing to the absence of the authors when the papers were called.

ON ELECTRICAL CONDUCTION *versus* INDUCTION. By A. E. Dolbear, of College Hill, Mass.

ON TELEGRAPHING WITHOUT WIRES. By A. E. Dolbear, of College Hill, Mass.

ON THE CONSTITUTION OF MAGNETS. By A. E. Dolbear, of College Hill, Mass.

ON VORTEX RING PHENOMENA. By A. E. Dolbear, of College Hill, Mass.

EXHIBITION OF A SIMPLE AND INEXPENSIVE COMPARATOR FOR MEASURING DISTANCES BETWEEN THE LIMITS OF 1 MM. AND 1 DM. By William A. Rogers, of Cambridge, Mass.

EXPERIMENTAL DETERMINATION OF THE LIMITS OF ACCURACY IN MEASUREMENTS BY THE SENSE OF FEELING. By W. A. Rogers, of Cambridge, Mass.

DETERMINATION OF THE RELATION: — METRE DES ARCHIVES = IMPERIAL YARD + 3.37015 INCHES. By W. A. Rogers, of Cambridge, Mass.

NOTE ON THE APPEARANCE OF A HALO ON THE EVENING OF AUGUST 4, 1882. By James D. Warner, of Brooklyn, N. Y.

ON SOME PHENOMENA OF DIFFRACTION DUE TO THE SHAPE OF THE SOURCE OF LIGHT. By Brown Ayers, of New Orleans, La.

THE INFLUENCE OF THE PHASES OF HARMONICS ON THE TIMBRE OF SOUND. By Rudolph Kœnig, of Paris, France.

ON THE REDUCTION OF THE ELECTRICAL RESISTANCE OF THE CARBON BUTTON BY THE PASSAGE OF AN ELECTRIC CURRENT. By T. C. Mendenhall, of Columbus, Ohio.

CONCAVE GRATINGS FOR USE IN SPECTROSCOPIC WORK. By H. A. Rowland, of Baltimore, Md.

ON THE "ROTATIONAL COËFFICIENT" IN GOLD, IRON, ETC. By Edwin H. Hall, of Cambridge, Mass.

ON THE DIATONIC SCALES OBTAINED IN THE CHROMATIC SCALES OF EQUAL TEMPERAMENT OF TWELVE, NINETEEN AND THIRTY-ONE TONES IN THE OCTAVE, WITH EXHIBITION OF NOVEL SCALE-INDICATOR AND CORRESPONDING KEYBOARD. By P. H. Van der Weyde, of New York, N. Y.

SECTION C.

CHEMISTRY.

ADDRESS

BY

H. CARRINGTON BOLTON,

VICE PRESIDENT, SECTION C.

FELLOW MEMBERS OF THE CHEMICAL SECTION;

Ladies and Gentlemen:—

THE recognition by the Association of the equal rights of chemical science and the elevation of the late "Permanent Subsection" to the dignity of a "Section of Chemistry," now assembled for the first time, mark an important epoch in the annals of our organization. Permit me to offer congratulations on our promotion and to express my high appreciation of the honor of presiding over your deliberations.

The Permanent Subsection of Chemistry was organized at the Hartford meeting of the Association in 1874. It had its origin in the action of a number of chemists assembled at Northumberland in August of the same year to commemorate Priestley's discovery of Oxygen. At that memorable gathering a discussion of the advantages of forming an independent organization in the interests of chemistry led to the appointment of a committee* "to cooperate with the American Association for the Advancement of Science at their next meeting, to the end of establishing a chemical section on a firmer basis."

This committee met at Hartford a few weeks later and united with the chemical members of the Association in founding a Per-

*The committee consisted of Profs. Silliman, Smith, Horsford, Hunt and Bolton.

manent Subsection in accordance with the provisions of the new constitution adopted at the same meeting. Meetings of the subsection were held on two days and arrangements were made for its permanent organization. In the following year the Association met at Detroit and the Subsection re-assembled under the chairmanship of Prof. S. W. Johnson. Before adjourning the section elected Prof. Geo. F. Barker chairman for the ensuing year and passed a resolution requesting the chairman-elect to prepare an address. This was the origin of the custom which it is my pleasant duty to follow. Professor Barker accepted the task and his masterly essay on the Atom and the Molecule (1876) is remembered by many in this audience. Since then Prof. F. W. Clarke (1878) has urged in your hearing the endowment of laboratories of research; Prof. Ira Remsen (1879) has magnified the claims of his chosen field of investigation — organic chemistry; and Prof. J. M. Ordway (1880) has gracefully surveyed the experiences of the past, the needs of the present and the prospects of the future of chemistry.

The amendments to the Constitution, adopted by the Association at the Cincinnati meeting, provide, as you are aware, for nine sections, each with its own presiding officer; since this may involve an equal number of annual addresses, it is hardly to be expected that each will maintain the elaborate character of those of former years.

I ask you therefore to bear this fact in mind while I invite your attention to a rather superficial survey of

CHEMICAL LITERATURE.

The literature of chemistry, extending as it does through a period of more than fourteen centuries, varies greatly in character, in province and in design; it partakes of the peculiar phases exhibited by the science at different epochs and depicts the experiences and thoughts of those who cultivated it in all ages. It may be studied from several points of view: the biographer searches the voluminous records to acquire knowledge of the intellectual activity of individuals; the historian unfolds the progress made by the science in a special field or in its entirety, with philosophical inquiries respecting effects and causes; the bibliographer, scarcely penetrating beyond the title pages of the dusty tomes, laboriously catalogues them to facilitate the researches of others.

We do not propose to give you a biographical, an historical or a bibliographical treatise, but rather to review chemical writings as sources of information and as portions of the world's literary productions. We shall concern ourselves less with the questions what were the personal history and life-work of a given author, and more with the queries what are the characteristics of the various classes of works at different epochs, what discoveries do they chronicle and what was their influence on the contemporaneous science.

The very earliest information concerning chemical arts comes to us from that ancient nation supposed by some to have given its own name to the science itself; not only do the sculptured tombs and temples of Egypt portray with unimpeachable authenticity and wonderful accuracy the technical skill of that venerable people, but these same monuments are even now relinquishing their hold on long-buried treasures in the form of papyri, whose perplexing script no longer conceals their meaning from the erudition of Egyptologists.

Of these miraculously preserved papyri the most valuable to chemistry is that discovered by Prof. George Ebers at Thebes in 1872, and named after its learned discoverer. We have described this elsewhere[1] and shall not here enter into details. It is the most ancient medical work extant, being assigned to the sixteenth century B. C., and contains a vast amount of information on the medical practice and the pharmaceutical preparations at that remote period. The unknown author wrote less obscurely than many of a much later date, and when the whole papyrus shall have been deciphered it will prove an invaluable contribution to chemical history.

The most ancient manuscript treating exclusively of chemical operations is a Greek papyrus of Egyptian origin preserved in the Library of the University of Leyden. Its authorship is unknown, its date is placed by Reuvens in the third or fourth century A. D. This MS. consists of a collection of prescriptions and receipts for conducting various operations in metallic chemistry, such as the testing of gold and silver; the purification of lead, of tin and of silver; the hardening of tin and of silver; the albification of copper, etc. It deals little with alchemy though some of the receipts evidently refer to transmutations, as those entitled: "the prepara-

tion (artificial?) of silver;" "the preparation of gold;" "the purification of tin by silver," etc.

Reference is made to sandarach (realgar), cadmia (zinc ore), chrysocolla, cinnabar, natron (soda), mercury and other chemical substances, but no receipts are given for their preparation. The author quotes from the Materia Medica of Dioscorides who probably preceded him by about two centuries. It is to be regretted that the full text of this ancient manuscript has never been published; the little known of it foreshadows information of great interest.[2]

The great libraries of Paris, Rome, Venice, Milan, Escurial, Cracow, Gotha, Munich and Cologne preserve a large number of Greek alchemical manuscripts of unknown authorship and uncertain date. Hoefer, the French historian of chemistry, refers them to the third and fourth centuries,[3] but other authorities with greater probability place them not earlier than the tenth and eleventh.[4]

The most celebrated of these essays are attributed to Zosimus, of whose history nothing is certainly known, and bear these titles: "On Furnaces and Chemical Instruments," "On the Virtue and Composition of Waters," "On the Holy Water," "On the Sacred Art of Making Gold and Silver." In a treatise attributed to Synesius, we find a description of a hydroscopium or hydrometer which was rediscovered as long after as the sixteenth century.

In a treatise attributed to Olympiodorus, he cites as authorities Democritus, Anaximander, Zosimus, Pelagius, and Marie a certain Jewess whom the later alchemists confounded with Miriam, Moses' sister.

In these manuscripts chemistry is called the "sacred art" and the exceedingly obscure and figurative language in which they are written makes it well nigh impossible to separate fact from fancy; Hoefer has indeed attempted to discover modern chemical conceptions in the allusions to Egyptian myths and the chaotic collections of spagyric arcana.

Of systematic nomenclature there is absolutely no trace; indeed each author seems to have aimed to write treatises intelligible only to himself, and we greatly doubt his success in even this respect. "Cadmia," we are informed, "is magnesia," and "magnesia is the female antimony of Macedonia;" "nitre is white sulphur which produces brass;" equally clear is the statement that the

"apospermatism of the dragon is the mercury of cinabarn." That lexicons were early in demand is not surprising; in fact some of the most ancient MSS. are "vocabularies of the sacred art," but even with their assistance it is difficult to form satisfactory concepts of contemporary chemical science.

Suidas,[5] a Greek lexicographer of the eleventh century, states that Diocletian having conquered the rebellious Egyptians (296 A. D.) destroyed their books on the preparation of silver and gold, lest becoming rich by the practice of that art they might again resist the Romans. Regrets at the wanton acts of this imperial biblioclast are tempered by the reflection that modern scholars are spared the study of such literary absurdities.

The Chinese, that curious people who always claim a hearing when the origin or antiquity of arts and sciences is under consideration, were acquainted at a very remote period with many branches of chemical technology. We do not know of any special chemical literature produced by them, but the researches of Rev. Joseph Edkins[6] and of Dr. W. A. P. Martin[7] make it highly probable that scholars will yet discover contributions of no small importance to the early history of chemistry. Prof. George Gladstone[8] has endeavored to show that the Chinese originated the doctrines and pursuit of alchemy and communicated it to the Arabians by whom it was disseminated throughout Europe.

The high state of civilization and extraordinary intellectual development of the Arabians has left a deep impression on chemical science. Cultivated chiefly by physicians, attention was directed to its pharmaceutical applications, and in spite of the prohibitions of the Koran to the fascinations of alchemy. Of their extant writings, preserved in European libraries, only a portion have been edited; those best known partake of the poetical imagery and hyperbole characteristic of the Oriental mind. This is shown to some extent in the singular titles prefixed to their treatises, *e. g.*, "The Rise of the Moon under the Auspices of Golden Particles," by the alchemist Dschildegi; "A poem in the Praise of God, of Mahomet and of Alchemy," by Dul-nun-el-Misri.[9]

The well known treatises of Geber,[10] "Of the Investigation of Perfection," "Of the Sum of Perfection," "Of the Invention of Verity," and "Of Furnaces," notwithstanding a bewildering style of composition, which seems to confirm Dr. Johnson's derivation of

gibberish, from Geber, display very great familiarity with a large number of chemical substances and operations.

Geber's works are generally assigned to the eighth century and consist chiefly of compilations from the "Books of the Ancients;" he mentions no author by name. They contain chapters devoted to the seven known metals, to the methods of distillation, calcination, cupellation and other operations, to the preparation of saline substances and to chemical philosophy. Geber adopted Aristotle's views of the constitution of matter from four principles, the hot and cold, the wet and the dry, and adds thereto: "Mercury and sulphur are the components of metals," a doctrine which with slight modifications prevailed for more than eight centuries. Geber describes the preparation of nitric acid, of aqua regia, and of mercuric oxide; he mentions the increase in weight of metals when calcined with sulphur, and gives the results of a rude quantitative analysis of crude sulphur. He constantly maintains the doctrine of transmutation of metals and gives a refutation of the ingenious arguments opposed thereto. His remarks on the qualifications of a chemist are most intelligent and are not inopportune in modern times; he urges the necessity of diligence, patience, learning, a temperate disposition, slowness to anger, and a full purse, "for this science agrees not well with a man poor and indigent," together with faith in the God who "withholds or gives to whom he will" the secrets of nature, and who will infallibly punish the foolish meddler with magical mysteries.

To detail fully our obligations to Arabian chemists is no part of our plan. They have left an indelible impression on the very language of the science, in the words alcohol, alembic, alkali, borax, and many others. All honor to the intelligent authors who a thousand years ago defined chemistry as the "Science of Combustion, the Science of Weight, the Science of the Balance!" [11]

In the middle ages intellectual activity was confined largely to the clergy, who controlled the schools of learning, the libraries, and nearly all sources of knowledge. University chairs were occupied exclusively by clerical professors[12], literature and science were cast in ecclesiastical moulds. Scientific treatises were the production of monks and emanated from cloisters. Many distinguished philosophers mastered widely separated branches of learning: among these were Alain de Lille (b. 1114), celebrated as a

physician, theologian, poet and historian, who filled the episcopal chair at Auxerre; Roger Bacon (b. 1214) an English cordelier; Raymond Lully (b. 1235), a Franciscan friar, and Albertus Magnus (b. 1193), Bishop of Ratisbon. The latter, amid the monotonous routine of a Dominican monastery, found leisure to distinguish himself in astronomy, medicine, alchemy and, according to his enemies, in necromancy. At this remote period, accusations of dealing with magic were not unfrequently made against those whose learning and skill in experimental sciences excited envy and superstitious zeal.[13]

To treat the writings of these eminent ecclesiastics as a part of chemical literature requires perhaps a stretch of the imagination, yet three hundred years ago they were regarded as masterpieces of the science and formed the text-books of students of alchemy. The writings of these ecclesiastical philosophers are as comprehensive as the branches of learning they cultivated, and incredibly voluminous; Albertus Magnus' collected works fill twenty-one folio volumes.[14] But a small fraction of these treatises are occupied with science and chemistry; and of this fraction there is in many cases a reasonable doubt as to their authenticity. In fact, nothing was more common than the ascription of work by an obscure second-rate writer to some celebrated philosopher of preceding ages, in order to give the work the stamp of authority,—a deception which previous to the invention of printing was more readily accomplished.

It became difficult therefore to distinguish the apocryphal writings from the genuine. The former, it is true, frequently betray themselves by anachronisms and other blunders, but many ingenious writers avoided such traps by adopting an enigmatical style worthy of the Delphian oracles.

Basil Valentine was the reputed author of works held in the very highest esteem by the alchemists of the Middle Ages, yet the very existence of this individual is seriously questioned. Mystery surrounds Valentine's entire history, and his writings were given to the world in a most dramatic manner; according to tradition they were hidden in the wall of a church at Erfurt and long after his death a thunderbolt shattered the wall and revealed the precious documents.

Whether Valentine was a real personage or not the works ascribed to him exhibit great familiarity with many chemical sub-

stances and operations, though the obscure and incoherent style renders their intelligent perusal very difficult.

Valentine's celebrated "Chariot of Antimony,"[15] extolling the medical virtues of this metal, is perhaps the least obscure of his works; the "Twelve Keys of Philosophy"[16] with its singular plates, one of the most unintelligible; yet beneath the extravagant jargon characteristic of the period, glimpses are obtained of light and intelligence. The latter work presents clearly the theory that all metals are compounded of three principles: fixedness, metallicity and volatility, represented respectively by salt, mercury and sulphur, an hypothesis which long completely controlled chemistry until it gave place to the seductive theory of Phlogiston. It is uncertain whether the works ascribed to Valentine were first written in Latin or in German; his writings were collected in the seventeenth century and have been through many editions.[17] Several of his treatises have been translated into English and into French.[18]

In the fifteenth century the newly invented printing press was employed in the production of few works which can be regarded as chemical, and these were chiefly confined to isolated treatises of the ancient philosophers; in the sixteenth century the alchemists began to publish the results of their industry and speculations, and in the succeeding century a prodigious number of alchemical works were issued in Germany, France and England, creating literature of an extraordinary type.

Some of these treatises, which are numbered by thousands, record valuable experiments made by enthusiasts seeking the philosopher's stone, but the majority contain "a crude mass of incoherent propositions and wild assertions, a mixture of poesy and insanity, in which all logical ideas are lost amidst the stilted phraseology, but through which breathed a blind yet fervent faith."[19] Great obscurity of style[20]; an enigmatical method of naming chemical substances which found its highest development in the use of arbitrary symbols and the pictorial representations of alchemical processes[21]; the intimate association with astrology; the honest or affected intermingling of pious comments and prayers[22]; the extravagant claims to antiquity as respects authorship and processes[23]; the attempts to interpret the mythology of Egypt and Greece on an alchemical basis; the endeavor to associate the mysteries of Hermes with the sacred truths of the Christian religion[24],— all combine to produce literary monstrosities as fascinating to the student of chem-

ical history as they are profitless to the practical worker in modern science.

Among the fabulous writings, highly esteemed by the credulous alchemists, may be mentioned the celebrated inscription of Hermes Trismegistus upon an Emerald Tablet[25], the Golden Leaves of Abraham, Jew Prince, Priest, Levite, Astrologer and Philosopher, which in the hands of Nicolas Flamel[26] yielded such a rich harvest, the Practical Chemistry of Miriam the sister of Moses[27], and a multitude of grotesque writings ascribed to personages of known reputation. Raymond Lully is credited with five hundred works; Hermes Trismegistus, the mythical Father of Sciences, with several thousand.

Many of the alchemical works published during the period of which we speak are degraded by admixture of the contemporaneous pseudo-sciences, judicial astrology and magic. To this class belong the celebrated works of Dr. Fludd and the writings of the Rosicrucians; excluding these as wholly beneath our consideration the number of occult works on alchemy is still very large. We imagine it will be hard to discover in the whole range of literature writings having scientific pretensions more senseless than the aphorisms of the disciples of Pythagoras, collected in the "Turba Philosophorum"[28] so often quoted by the alchemists of the sixteenth and seventeenth centuries. Its improbable character is perhaps equalled by the "Gloria Mundi" in which the anonymous author favors his readers with the chemical views of Aristotle, Plato, Socrates and Democritus, interspersed with equally authentic statements by Hermes and Morien, Lamech and Methuselah, Abel and Seth, and even of Adam himself.[29]

In the early part of the seventeenth century, Michæl Maier, physician to the Emperor of Germany, Rudolph II, a royal patron of astrologers and alchemists, published several treatises now much sought after by alchemical bibliophiles.[30] Maier's "Symbola aurea mensæ" and "Atalanta Fugiens" contain emblematic plates, supposed to illustrate the hermetic interpretation of the fables and allegories of Egypt and Greece.

The connection between these ancient mythologies and the secrets of the philosopher's stone was a favorite subject with many authors and has been exhaustively treated by the Abbé Pernety in his two curious works "Fables Egyptiennes et Grecques dévoilées" and "Dictionnaire Mytho-hermétique."[31] Not content with prose, several authors clothed their alchemical inspirations in poetry;

of these may be mentioned the Arabian treatises already named, the Twelve Gates of Alchemy by Sir George Ripley, written about 1450[32], the Crede Mihi of Thomas Norton, written about 1477[33] and the Chrysopœia of Aurelius Augurelli.[34] The latter written in Latin hexameters with more pretensions to elegance than usual with the prosaic alchemists was dedicated in 1514 to Pope Leo X. Leo rewarded Augurelli by presenting him with an empty wallet, remarking that one who knew so well how to make gold had need only of a purse.

The alchemists, in common with their contemporaries in other branches of literature, took pleasure in prefixing to their essays eccentric titles; Helvetius writes of "The Brief of the Golden Calf (The World's Idol) discovering the rarest Miracle in Nature;"[35] Glauber names one of his treatises, "The Golden Ass well managed and Midas restored to reason."[36]

Perhaps the height of absurdity is reached in the famous *Liber Mutus*[37], which consists of a series of fifteen symbolical engravings purporting to disclose the whole Hermetic Philosophy. The utterly unintelligible character of much alchemical literature is occasionally acknowledged by those who otherwise accepted the prevailing popular belief in transmutation; Lenglet du Fresnoy, speaking of Raymond Lully's Clavicula, says "Lully assures us that this treatise is indispensable to the comprehension of his writings; but, after reading it, one is little wiser than before."[38]

There are several large collections of alchemical treatises which the curious in these matters should consult; the most extensive is Zetzner's Theatrum Chemicum[39] published in 1613 in six octavo volumes; this contains no less than 209 distinct treatises, notwithstanding which Lenglet du Fresnoy remarks that many excellent ones are wanting. Manget's Bibliotheca Chemica Curiosa,[40] published in 1702 in two folio volumes, contains 133 treatises; and the Musæum Hermeticum[41] (1678) contains 21 treatises, some of which are illustrated. The most extensive English collection is Ashmole's Theatrum Chimicum Britannicum, published in 1652, and a noted French collection is Salmon's Bibliothèque des Philosophes Chimiques (1672), of which the edition by Richebourg (1741) is by far the best.[42]

Stimulated by avaricious hopes, the zealous alchemists labored most industriously; and, by subjecting mixtures of all known substances to heat and to the action of acids, discovered a multitude of new bodies having more or less medical and economic value.

As we have seen, their writings consist for the most part of monographs describing disconnected experiments, with no attempt at exhaustive treatment of any single topic, no classification of phenomena and no well studied arrangement of material such as characterizes later handbooks. Nor can a scientific collocation be expected during the formative period of chemistry and previous to the introduction of theories around which to group the isolated phenomena. One of the earliest attempts to treat chemical facts in a systematic manner was made by Sir George Ripley, Canon of Bridlington, who lived in the fifteenth century. In his "Compound of Alchemy" written in 1471 the whole science of hermetic chemistry is unfolded in a poem divided into twelve sections called "Gates" through which the reader is conducted to the mysteries of transmutation.[43]

"But into chapters thys Treatis I shall devyde,
In numbre twelve, with dew recapytulatyon;
Superfluous rehearsalls I lay asyde,
Intendyng only to give trew informatyon
Both of the theoryke and practicall operatyon:
That by my wrytyng who so wyll guyded be,
Of hys intente perfyctly speed shall he.

The fyrst chapter shall be of natural Calcination;
The second of Dyssolution, secret and phylosophycall
The third of our elementall Separation;
The fourth of Conjunction matrimonial;
The fyfth of Putrefaction then followe shall;
Of Congelation Albyficative shall be the sixt,
Then of Cybation, the seaventh shall follow next.

The secret of our Sublymation the eyght shall show;
The nynth shall be of Fermentatyon;
The tenth of our Exaltation I trow.
The elevent of our mervelose Multiplycatyon,
The twelfth of Projection; then Recapitulatyon,
And so this treatise shall take an end,
By the help of God, as I entend.

. . . . . .

Thus here the Tract of Alchemie doth end;
Which tract was by George Ripley, Chanon, penn'd
It was composed, writt and signed his owne,
In anno twice seaven hundred seaventy one.
Reader, assist him, make it thy desire,
That after life he may have gentle fire! Amen.

Ripley's versification and theme remind one of Chaucer's Canon's Yeoman's Tale composed nearly one hundred years earlier.

We can only briefly allude to Paracelsus' bombastic productions[44], which are concerned chiefly with medical chemistry, and to the remarkable works of George Agricola[45], distinguished by profuse illustration, portraying the apparatus and operations of mining and metallurgy in the sixteenth century.

Conspicuous for accuracy of description and systematic arrangement of topics is the "Alchymia" of Andrew Libavius, published at Frankfurt in 1595.[46] Libavius, a physician and teacher in the gymnasium at Coburg, rejected the absurd doctrines of the adherents of Paracelsus, combated superstition and quackery and, excelling in observation of chemical phenomena, gained a worthy position among his compeers. In his Alchymia he treats of the *Encheria* or manual operations of chemistry and of the *Chymia* or descriptions of substances, in separate books. The former he divides into two sections, one dealing with the instruments and the other with the management of fires and construction of furnaces. He describes at length a sumptuous laboratory provided not only with every requisite for chemical experimentation, but also with means of entertaining visiting guests, including such luxuries as baths, enclosed corridors for exercise in inclement weather, and a well stocked wine-cellar.

Libavius was the discoverer of stannic chloride which still bears the name "fuming liquor of Libavius"; he describes the method of preparing artificial gems by coloring glass with divers metallic oxides, and he seems to have been the first to apply the balance to the examination of mineral waters. Notwithstanding his progressive position he devotes eighty pages of his Commentary to the Philosopher's Stone in which he was a firm believer. The second edition of his works, published in 1606, forms a folio of 800 pages. It has sometimes been called the First Text-book of Chemistry.

The "Tyrocinium Chymicum" (1608) of Jean Beguin[47], Almoner to King Louis XIII of France, a less pretentious work, is characterized by freedom from ambiguity and prolixity as well as from hermetic superstitions; it deals chiefly with the medical applications of chemistry.

In 1660, Nicolas le Febvre, demonstrator of chemistry at the Jardin des Plantes, published a "Traicté de la Chymie"[48] greatly superior to all preceding works of its kind. He collected the most reliable theories and experiments from all published sources

and arranged them in a logical, systematic manner. The first six chapters treat of the theory of chemistry; the author admits five elementary principles: phlegm (or water), spirit (or mercury), sulphur, salt and earth, the first three being ingredients of volatile substances, and the latter those of refractory bodies. He defines metals as hard bodies generated in the bowels of the earth, capable of extension under the hammer and of being melted by fire. He divides metals into the perfect and the imperfect, and also into male and female, the latter subdivision being based upon their behavior with acids; gold, lead and antimony are male metals because soluble only in aqua regia, and the other five, silver, copper, iron, tin and mercury are female, because soluble in unmixed acids.

In the second part the author discusses the manipulations of chemistry and recognizes many nice distinctions now overlooked; for example, under the head of mechanical division he describes in detail the following operations: "limation, rasion, pulverization, alcoholization, incision, granulation, lamination, putrefaction, fermentation, maceration, fumigation both dry and humid, cohobation, precipitation, amalgamation, distillation, rectification, sublimation, calcination both actual and potential, vitrification, projection, lapidification, extinction, fusion, liquation, cementation, stratification, reverberation, fulmination or detonation, extraction, expression, incineration, exhalation, digestion, evaporation, desiccation, circulation, congelation, crystallization, fixation, volatilization, spiritualization, corporification, mortification and revivification."

In the experimental part Le Febvre explains the arrangement in the following language:

"We shall begin with the meteoric bodies, rain, dew, honey, wax and manna; we shall then describe the preparations made from animals and their secretions; next, the numerous products of the vegetable world; and lastly, the mineral kingdom with its stones, salts, marcasites and metals;" a division still recognized.

The order in which he treats mineral chemistry is as follows: Earths; stones, precious and otherwise; metals; semi-metals, embracing mercury, antimony and bismuth; salts, including common salt, saltpetre, alum, sal-ammoniac and the vitriols; and lastly, the sulphuretted minerals, arsenic, etc. Under each division he gives precise instructions for numerous experiments upon these

substances and describes their medicinal value. The whole work is written in a clear style, free from affectation of mystery; it rapidly passed through five editions, and was translated into English and German.

Three years after the appearance of Le Febvre's Traité, his successor at the Jardin des Plantes, Christopher Glaser[49] of Basle, published a work having the same title also marked by special excellence.

The most successful handbook of the seventeenth century was undoubtedly the "Cours de Chymie" by Nicolas Lemery,[50] an eminent lecturer in Paris. The first edition of this work was published in 1675, and it reached the tenth edition before the close of the century; a fourteenth edition enlarged by Baron appearing as late as 1756. It was also translated into English (1677), German (1698), Italian (1763), and Spanish. The remarkable success of this work is due to a facility for describing dry facts with remarkable simplicity and accuracy. His style is more concise than Le Febvre's, and his arrangement of material shows a progressive spirit. The limits of this address forbid an analysis of Lemery's Handbook, which, moreover, is better known than some others to which we have granted fuller treatment.

Passing over a period of fifty years, the next complete compendium of chemical knowledge which we notice is the "Elementa Chemiæ" of Hermann Boerhaave, the celebrated professor of botany, chemistry and medicine in the University of Leyden. Boerhaave's great erudition, purity of style and brilliant eloquence attracted students in great numbers; such was the popularity of his lectures that "certain booksellers who aimed at lucre by the most scandalous means" published them in 1724, without his authority or consent; this "surreptitious edition"[51] contained, as Boerhaave himself complains, such "false, ridiculous and absurd things in every page," that he was compelled to publish his lectures in an accurate and complete form. Both editions were translated into English by Dr. Peter Shaw, and the authorized work passed through many editions in Latin, French and German.[52] Instead of attempting to give an analysis of two quarto volumes, we will quote Boerhaave's own account of his plan.

"My design," he says, "is to initiate students in the knowledge of chemistry; and to do this in the most effectual manner, I

shall give a clear methodical explication of all that is necessary for understanding the best authors and for performing the chief operations in this experimental art." After acknowledging the difficulty of a systematic treatment of "a science which has been cultivated rather by experiments at random than upon any regular principles, and by persons usually destitute of all . . . knowledge in the liberal arts;" he claims that "these obstacles may be surmounted by making a collection of the several effects, which the art has actually produced, justly deducing general rules therefrom and duly digesting the whole." He then explains the division of his work into three parts.

"The first will rehearse the origin, progress, cultivation and fortune of chemistry, . . ; the second part will deliver certain theorems or principles of chemistry; the third will exhibit the actual operations of chemistry, whereby bodies are changed agreeably to the rules of the art and to the end proposed therein." Boerhaave's sketch of the history of chemistry begins with the earliest times but seems to have been left incomplete; it is rendered useful by classified lists of chemical writings and numerous references.

In his treatment of the theory he departs somewhat from his plan and introduces material which really belongs to the third or practical part. Boerhaave introduces sections on the use of chemistry in natural philosophy, in medicine and in the mechanical arts; gives an exhaustive account of the wonderful nature of fire and experiments in heat, together with ingenious speculations concerning the heat of celestial bodies; he describes the various forms of chemical apparatus and the preparation of curious and useful substances from the vegetable, animal and mineral kingdoms. Boerhaave exhibited the spirit of a true philosopher and produced a work of extraordinary merit. He was quite free from the follies of alchemy, though he cautiously remarks that "we should always remember the limits of nature are by no means to be defined by us, things are taken for impossible which are only unknown by the ignorant" and "many things in chemistry are apparently more incredible than that lead should lose its natural form and be converted into gold." [53]

Contemporary with the systematic compilations of Libavius, Le Febvre, Boerhaave and others, were published hosts of independent works setting forth the results of prodigious labor in the chemical laboratory; many of these are filled with descriptions of

experiments made at haphazard, with no definite object in view, and having no necessary connection with each other.

Glauber's[54] voluminous writings, published in 1658, may be taken as an example of this class; in his works, amid controversies with Galenical physicians and curious apologies resulting therefrom, amid descriptions of alchemical mysteries and receipts for a universal panacea, amid extravagant praises of the "sal mirabile" (Glauber's salt), and curious narratives of personal history, we find many novel chemical facts having a medical or an industrial value. The whole is clothed in a very crude style with an affectation of secrecy and under capricious captions.

On the other hand, some of these treatises show signs of genius, especially in attempts to establish theories in explanation of the familiar yet astonishing phenomena; thus Van Helmont, whose contributions to medical chemistry we pass by, invented the word *gas* to aid in discriminating aeriform bodies[55]; and Rey, in his "Essays on the increase in weight of tin and lead when calcined[56]," demonstrated the weight of the air thirty years before the masterly researches of Boyle.

To attempt in this rapid review to do justice to the philosophical writings of the Hon. Robert Boyle is a hopeless undertaking. From his first publication, in 1660, "New experiments, physico-mechanical, touching the spring of the air and its effects," to his posthumous treatise "Medicinal Experiments," published in 1692, his works teem with ingenious experiments described with great candor and fidelity, and from which conclusions are drawn with cautious reserve and philosophical soundness, as admirable as his rare modesty.

In his "Sceptical Chymist" (London, 1661), Boyle raises doubts as to the elementary nature of earth, air, fire and water, as well as of the alchemical principles, sulphur, salt and mercury, and claims that the first elements of bodies are atoms of different shapes and sizes, the union of which forms what are vulgarly called elements; he argues that their number should not be confined to three nor four, but that probably a large number of primary constituents would eventually be separated or isolated as such. He also first clearly recognizes the distinction between mixture and chemical combination.

The rejection of Boyle's prophetic hypothesis by his contemporaries much retarded the progress of the science. "More, how-

ever, than for his views on the nature of the elements, science is indebted to Boyle for his clear statement of the value of scientific investigations for its own sake, altogether independent of any application for the purposes either of the alchemist or of the physician . . Boyle was in fact the first true scientific chemist, and with him we may date the commencement of a new era for our science when the highest aim of chemical research was acknowledged to be the simple advancement of natural knowledge." (Roscoe.)

Boyle's voluminous writings were collected by Birch in five folio volumes and published in 1744. His diffuse style of composition, with frequent long digressions, renders a perusal of his writings wearisome and led to an edition by Dr. Shaw, in which his works are "abridged, methodized and disposed under general heads," forming three quarto volumes bearing the date 1725.[57] Many of Boyle's papers were published in the Philosophical Transactions.

The literature pertaining to the theory of phlogiston belongs almost wholly to the eighteenth century. It is true that Becher's Physica Subterranea was published in 1659[58], but the theory of a combustible principle existing in all metals remained dormant until Becher's admirer, George Ernest Stahl, published an edition of the Physica Subterranea in 1702; to this he prefixed a long preface elaborating those doctrines which exerted such immense influence on both the theoretical and practical science, for more than a century. The writings of Becher and Stahl are notorious for their barbarous mixture of poor Latin and German, for which indeed both authors apologize.[59]

In Mayow's Treatise on the nitro-aërial spirit[60], we find records of admirable experiments in pneumatic chemistry supported by accurate reasoning and almost prophetic insight of later theories of combustion, but his early death prevented the continued support of his views, and Mayow's treatise was soon forgotten by the prejudiced followers of Stahl.

In the year 1683 a noteworthy event occurred in the chemical world, the opening of public laboratories of instruction. Chairs of chemistry had long existed in institutions of learning, the first being filled nearly eighty years before by Johann Hartmann, and the honor belonging to the University of Marburg. Practical in-

struction had been secured also in the private laboratories of the wealthy, thus Homberg and Friedrich Hofmann worked in Boyle's establishment[61]; but the first public laboratory for instruction was opened at Altdorf, Bavaria, under the direction of Prof. J. M. Hofmann. A survey of the status of chemical knowledge at that date offers a tempting digression but the length of this address forbids.

The second public laboratory was opened in the same year (1683) at Stockholm, under the patronage of Karl XI of Sweden, and under the guidance of Urban Hiärne. Both institutions issued publications bearing the general title Acta[62] which may be regarded as forerunners of the "Contributions from the Laboratory of ——— University" now so common in periodical literature.

Lexicons and dictionaries have been a feature of chemical literature from the earliest times; we have already alluded to the vocabularies of the sacred art found among the Greek MSS. of the Paris National Library and referred to the tenth and eleventh centuries. These, however, seem to have been compiled for the purpose of misleading the novice in alchemical mysteries, and as Hoefer justly remarks themselves need commentaries to become intelligible.

The universal employment of symbolic characters, to represent both chemical substances and operations, rendered alchemical literature difficult of perusal and early led to the publication of keys and vocabularies explaining them. Such keys occur in the manuscripts just mentioned, in Eschenreuter's "Universal Panacea for Men and Metals"[63] which purports to have been written in or before the fourteenth century, and in the works of Oswald Crollius[64], Kircher[65], Le Febvre, Lemery and many authors of the seventeenth century: these however are not properly dictionaries.

Dr. Martin Ruland, court physician to Rudolph II, published a "Lexicon alchemiæ" in 1612, and William Johnson[66] a "Lexicon chymicum" in 1657, but the first really satisfactory treatment of chemistry in monographs alphabetically arranged was by Macquer[67] whose "Dictionnaire de Chymie," published in 1766, had great success, passing through several editions and being translated into the German, English, Italian, Spanish, Danish and Russian languages.[68] Macquer found many followers, among whom

may be mentioned Nicholson (1795), Cadet (1803), and well known authors whose works are indispensable to all students of the science.

Prof. Wm. Ripley Nichols, of Boston, late chairman of this Section, in a recent private communication, has expressed the need of yet another dictionary, one of chemical synonymes. He calls attention to the large number of names by which certain chemical substances have been known at different epochs and in different professions, and to the utility of a dictionary explaining their synonymy. Professor Nichols suggests "restricting the list to terms found in the literature of the preceding hundred years, but including such alchemical names as have peculiar historic interest and such as have persisted more or less in commerce and pharmacy." He also suggests that the work should include English, French and German names, except when they are simply literal renderings of the same term, and names of those minerals which are of simple composition like halite, witherite, fluorite, etc., under their appropriate heading. In an example of the proposed method of treatment he gives a long list of names for ferric oxide under the several heads: alchemical, chemical, mineralogical, pharmaceutical, commercial and popular.

The idea is certainly excellent and we have Professor Nichols' permission to mention it publicly in the hope that some one may be incited to compile such a work. We are not aware of the existence of any modern chemical synonymicon, not having seen Berta's Dictionary of chemical terms[69], published at Padua in 1842; perhaps the nearest approach to the ideal work of Professor Nichols is Sommerhoff's Lexicon pharmaceutico-chymicum[70], published in 1701, a folio of 400 pages.

About the middle of the seventeenth century the dissemination of the views of Lord Bacon, as expressed in his *Novum Organon*, gave a great impulse to scientific investigations, and the "splendid fiction of the new Atlantis" was practically realized in the foundation of the "Royal Society for improving Natural Knowledge." The learned men, who in 1645 met in London, and there disturbed by unhappy dissensions of civil war withdrew to Oxford to report and discuss philosophical experiments, laid the foundations of an edifice destined to rise higher, endure longer and to shelter a nobler offspring than the most sanguine could have foreseen. The organization was granted a charter by King Charles II in 1662 and

three years later began the publication of the "Philosophical Transactions, giving some accompt of the present undertakings, studies and labours of the ingenious in many considerable parts of the world." In the following year the French Academy of Sciences began their Mémoires. Thus arose the element of periodicity in scientific literature, one which rapidly increased in importance.

The year 1665 also witnessed the birth of the parent of that large class of periodicals not issued by societies but published by private parties either individually or coöperatively. The first number of the "Journal des Scavans" appeared January 5, 1665, and for one hundred and thirty years was the most prominent literary and scientific journal of Europe. At that early date the periodicals partook of a literary nature rather than of the scientific, but gradually these two elements became distinct and the lines became more and more closely drawn until every branch of pure and applied science supported a serial especially devoted to its progress. Among the earliest periodicals devoted to chemistry and its associate physics may be mentioned the "Journal de Physique et de chimie," edited by the Abbé Rozier, de la Méthérie and others, begun in 1770 and continued through ninety-five volumes to 1822; the "Chemisches Journal" of Lorenz Crell, 1778–86, which was followed by the "Chemische Annalen" by the same editor, extending until 1803. The "Annales de Chimie et de Physique," begun in 1789, enjoys the honor of being the oldest surviving serial of this class.[71]

Of the foundation of Societies exclusively devoted to chemistry in France, England, Germany and America, and of their periodical publications, we can make mere mention. The existence of a "Columbian Chemical Society" in Philadelphia, as early as 1811, deserves passing notice. Two volumes of its Memoirs were published containing articles by Drs. Mitchell, Cutbush, Manners, Bache and others.

Of chemical bibliography and works relating exclusively to the history of the science, we have elsewhere[72] given a critical résumé.

As we proceed with this review of chemical literature, the publications increase enormously in number, variety and importance, and the end of our undertaking seems to move forward faster than we progress. It is manifestly impossible in the brief time allotted to this address to analyze the chemical literature of the preceding hundred years. How shall we attempt to portray the

theories and experiments recorded in the writings of Bergmann[73], Scheele[74] and Gahn, the eminent Swedes; of Black[75], Cavendish[76] and Priestley[77], the English triumvirate; of the French philosophers Fourcroy[78], de Morveau[79], and Lavoisier[80]; of the industrious German chemists Kunckel[81], Marggraf[82], Wenzel[83], Klaproth[84] and Richter[85]; or of the Italian philosophers Galvani[86], Volta[87], and Brugnatelli?[88] And passing to a later period, how shall we do justice to the labors of Berthollet[89], Gay Lussac[90], Thénard[91] and Laurent[92]; of the immortal Dalton[93], of the brilliant Davy[94] of the indefatigable analyst Berzelius[95]; of Mitscherlich, Liebig, Wöhler, and Hofmann; of Dumas, Berthelot and Adolph Wurtz; of Graham, Frankland, Abel and Crookes?

American contributions to chemical literature have been exhaustively discussed in the hearing of many present and need not now detain us; I refer to Prof. Silliman's Address at the celebration of the centenary of Priestley's discovery of Oxygen, held at Northumberland in 1874.[96]

The modern period of chemical literature is characterized by two opposing forces, a tendency to dispersal and an effort to collect the widely scattered publications. The multiplication of learned societies especially in Europe, each of which supports its own organ, and the increasing number of nations interested in scientific pursuits, in each of which arises an independent periodical, tend to the wide dispersal of memoirs, essays and notices.

On the other hand, industrious editors laboriously collect and set in order these scattered observations, constructing compact, massive handbooks and many volumed cyclopedias. The monumental work of Gmelin, which is even now passing through another edition, has worthy rivals in the magnificent compilations edited by Henry Watts and by Adolph Wurtz, and in the work of Beilstein in a field of special difficulty. For systematic treatises we can point to Miller, Graham-Otto, Pelouze et Frémy, Roscoe and Schorlemmer and the ever youthful Fownes.

Invaluable, too, are the annuals known as Jahresberichte, monuments of conscientious editorship.

Another feature of modern growth is the multiplication of special treatises dealing with single topics in every branch of chemistry, inorganic, organic, theoretical, analytical, technological, pharmaceutical and physiological, until mere acquaintance with their titles

becomes a serious undertaking for busy workers in the laboratory.

The amount of time and labor required to search for a given point throughout the maze of modern chemical journals, transactions, treatises and hand books, is well appreciated by my audience; the superb dictionaries and annuals referred to accomplish much in cataloguing, condensing and systematizing the infinity of observations; nevertheless, all working chemists feel the need of special indexes, simply arranged, as complete as possible and accessible to every one. Sharing in this feeling, I have contributed my mite to the literature of chemical indexes and have inspired several friends, fellow-members of this Association, with the courage to follow. Uranium[97], Manganese[98], Titanium[99], Vanadium[100], Ozone[101], and Peroxide of Hydrogen[102] have been indexed on a uniform plan, and my pupil W. W. Webb, B. S., has prepared an Index to the Literature of Electrolysis[103], which will soon be published in the same channel as the others;— the Annals of the N. Y. Academy of Sciences.

Pardon, we beg you, these personal allusions: we have indulged in them with the object of bringing before this Section a proposition for organized effort in the preparation of Indexes to all the Elementary Substances on a uniform plan.

The advantages which would accrue from the publication of special indexes to the literature of the elements and of such other subjects as may prove desirable are too obvious to need argument; I have thought that the undertaking might be placed in the hands of a Committee of the Chemical Section of this Association. The existence of an Index Society in England under the guidance of Henry B. Wheatley, F. S. A., Hon. Sec.,[104] affords a precedent, if indeed any be required.

The first duty of the Index-Committee of the Section of Chemistry would be to secure volunteers willing to undertake the compilation of special indexes; surely in this National Association a few chemists can be found ready to coöperate in such an important work. The Committee would further adopt a uniform plan for the indexes, determine the method or channel of their publication, and exercise general editorial supervision over the undertaking.

Hoping that this suggestion will receive your approbation I leave it in your hands.

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PAPERS READ.

On a New Microchemical Method of Determining the Feldspars in Rocks. By J. Szabó, of Budapest, Hungary.

My method of determining the feldspars in rocks is based chiefly on the coloration of the flame by potash and sodium, and on the degree of fusibility of the different kinds of feldspars.

In order to obtain the same results, the circumstances under which the experiments are made must be the same. The coloration of the flame and the fusibility are observed at the same time, in the same experiment.

I take a Bunsen burner of the dimensions described by Bunsen, the lamps being those furnished by Desaga in Heidelberg. I use two parts of the flame, one without the chimney at a height of five millimetres above the lamp for a lower temperature, and one for the highest temperature, which is produced by putting the chimney on the lamp and using a height of 5–10 millimetres above the chimney. It is the "Schmelzraum" of Bunsen, the region of fusion, and he estimates its temperature at 2300° C.

The platinum wire for introducing the feldspar grain must be very thin, a piece of one decimetre length should not weigh more than thirty-two milligrammes. I take pieces of it of three centimetres in length, connect them by fusion with a glass tube at one end, and at the other end, a ring is made of about one millimetre diameter, on which the grain of feldspar is fixed by means of distilled water.

The size of the feldspar must be approximately the same; it is the size of a fine mustard seed. If perfectly fused, it should be about this size (•). The feldspar grain is introduced into the different parts of the flame by fixing the glass tube to the holders used in the observations for spectrum analysis.

New determination of the fusibility of the minerals.

The result of determining the fusibility of the minerals being in many respects different from the results as obtained by Von Kobell, I find it necessary to give a brief description of my method of obtaining the degrees of fusibility.

For determining the fusibility with the same platinum wire and of the size mentioned, I use three parts of the Bunsen flame: *a*, the base of the flame; *b*, the height of five millimetres as mentioned, and *c*, the region of fusion, "Schmelzraum." Now I give the degrees of fusion according to the results obtained in those three experiments, and discriminate the following *eight* degrees for the fusibility of the minerals in general.

0. Infusible in all the three cases. Quartz, spinel.

1 (I = 0; II = 1) In five millimetres height during one minute no change is visible; but in the highest temperature after being exposed for one minute, the points and edges begin to be a little rounded, but on the sides no change is remarked. Bronzite, hematite, some anorthite, and some augite (Monté Rossi, Etna).

2 (I = 1; II = 2). The base of the flame serves in most cases only to fix the grain to the platinum wire. I speak of the first experiment, when the grain is introduced into the flame at five mm. height, and I call the second experiment obtained in the highest temperature. The mineral, to which I give the second degree of fusibility, shows after having been exposed for one minute to the flame in five mm. height a change in the points and edges, but the faces keep their original condition. In the second experiment even the faces began to fuse, but without being rounded. Some diallogite, bytownite.

3. (I = 1–2; II = 3). In the first experiment (five mm.; one minute) the points, edges and faces show the fusion but the general form of the grain is not changed. In the second experiment (region of fusion; one minute) the whole grain begins to be rounded, without becoming globular. Adularia (St. Gothard), labradorite, magnetite.

4. (I = 2–3; II = 4). In the first experiment (fifty-five mm.; one minute) it fuses without forming a globule; but in the second experiment it gives a perfect globule. Some hypersthenite, amphibole, most kinds of augite.

5. (I = 4). It fuses to a globule in the first experiment (five mm.; one minute); in this case the second experiment is not re-

quired. Petalite, garnet, some albite (of the trachytes of Hungary).

6. It fuses to a globule at the height of 5 mm. in less than one minute, or in the base of the flame but after the red heat. Borax, cryolite.

7. It fuses to a globule after some seconds near the base of the flame, before becoming red hot. Stibnite, sulphur, ozokerite, etc.

The results may be sometimes intermediate, for instance: 6–7 for rock salt, which begins to be fused at the commencement of a red heat; and the degree of fusion would be indicated as 5–6, 4–5, etc.

The exclusive use of Bunsen's burner in determining the fusibility is an advantage of my method as compared with the method of Plattner and Von Kobell, because the result is not affected by subjective ability of handling the blow-pipe. The temperature is nearly always the same, the gas flame used by me is never higher than fourteen centimetres. The time is also fixed, it is one minute.

The use of a lower and higher temperature and the observation of the change with a lens are in some cases very interesting. It is sometimes found that the substance is a mixture, for example, of feldspar and quartz; the feldspar is fusible, and the points and edges of the infusible quartz become visible in the mass in the first experiment, while at the high temperature the two substances may give one fused mass.

The chemical elements or the invariable binary compounds (quartz, magnetite, cassiterite, etc.) have a constant degree of fusibility, but this is not the case with compounds in which the isomorphic elements replace each other, or which are variable in the relative quantity of their elements, and that is the case with by far the greater number of minerals and especially with the feldspars. The presence of some elements make the mineral more or less fusible. Sodium is the element in the silicates which makes them easily fusible; the magnesium and aluminum render them less fusible, or altogether infusible. This important fact has induced me to establish the standard for the degree of fusibility according to the circumstances under which the fusion takes place, and not to take a mineral representing the degree, because such minerals are not to be found for all degrees among the elements, or the double constant compounds and the more complicated minerals, as amphibole, orthoclase, garnet, etc., may be variable, even when obtained from the same locality.

The fusibility of the feldspars, amphiboles, augites, as the chief rock-forming minerals, varies between 0–5.

The feldspars.

According to the dominant protoxide element there are potash, soda and lime-feldspars, which are passing into each other not only according to the silicic acid but also according to the protoxide element.

Tschermak discerns in the three kinds of feldspars on the "principle of equidistance" of the characteristic protoxide the following ten series.

A. Potash feldspar (*orthoclase*).

1. Adularia	series	K_2O	16–13 per cent
2. Amazonite	"	"	13–10 " "
3. Perthite	"	"	10–7 " "
4. Loxoclase	"	"	7–4 " "

B. Sodium feldspar (*sodium plagioclase*).

5. Albite	series	Na_2O	12–10 per cent
6. Oligoclase	"	"	10–8 " "

C. Lime feldspar (*calcium plagioclase*).

7. Andesine	series	Ca O	6–10 per cent
8. Labradorite	"	"	10–13 " "
9. Bytownite	"	"	13–17 " "
10. Anorthite	"	"	17–20 " "

If the dominating protoxide elements show this change, the other elements vary in their relative quantity, of which the limits are given in the following table, taken from a considerable number of reliable analyses.

	Na_2O	K_2O	Ca O	Al_2O_3	SiO_2
1. Adularia	1–2	16–13	0–15	17–19	67–64
2. Amazonite	2–4	13–10	0–15	17–19	67–64
3. Perthite	4–6	10–7	1–15	17–19	67–65
4. Loxoclase	6–8	7–4	1–15	19–20	67–65

	Na_2O	K_2O	CaO	Al_2O_3	SiO_2
5. Albite	12–10	0–1	0–1	18–20	69–66
6. Oligoclase	10–8	1–2	2–6	21–24	65–61
7. Andesine	8–5	2–1	6–10	24–29	60–54
8. Labradorite	5–3	2–1	10–13	25–30	56–50
9. Bytownite	3–1	1–0	13–17	29–34	52–47
10. Anorthite	2–0	1–0	17–20	32–37	46–43

If we take into consideration the results of the chemical, optical and crystallographical study of feldspars, we must distinguish: (*a*) in the class of potash feldspars, chemically, the adularia, amazonite, perthite and loxoclase; optically, orthoclase and microcline; (*b*) in the class of sodium feldspars, from a chemical point of view, albite and oligoclase; from an optical point of view, soda, plagioclase and the very rare soda orthoclase; (*c*) in the class of the lime feldspars the dimorphism has not yet been observed, therefore we discern only the four chemical species: andesine, labradorite, bytownite and anorthite, which on optical and crystallographical grounds are to be considered as four distinct species.

According to Tschermak's classification ten series have been established; there are intermediate series too, and in the same manner it is possible to distinguish less than ten series. From a geological point of view it is enough to establish the following five species: orthoclase, microcline, oligoclase, labradorite and anorthite.

The potash feldspar, as a rock-forming mineral, in most cases corresponds with the amazonite and perthite series, while adularia is restricted to the crystals found in veins and cavities of some orthoclastic rocks. It is therefore, for the geologist, sufficient to make use of the general name of orthoclase for the potash feldspar, except in the case when the microscope shows the presence of the dimorphic state, then the microcline must be named too. The microcline is very common in the old orthoclastic rocks, but in the newer volcanic rocks, containing the feldspars in a glassy state, I have never observed it.

In the series of the soda feldspars albite is restricted to the minerals found in the cavities of certain rocks, while the soda feldspar as a rock forming mineral is usually the oligoclase. In geological respect the oligoclase and the andesine have nearly the same

rôle, therefore generally they might be taken for one species; the distinction may be reserved for a more detailed classification.

The labradorite is the main representative of the soda lime feldspars, but variable too. It stands sometimes nearer to the andesine; at other times nearer to the bytownite series.

The true lime feldspar is anorthite with bytownite, which are nearly always found together.

In the same rock it is very rare to find only one kind of feldspar; there are at least two, if not more, to be found together, but one of them is usually dominant, and then it is entitled to enter into the nomenclature of the rock.

The coloration of the flame.

The yellow coloration of the flame by the feldspars is a constant character; I have never found a feldspar without sodium, though analyses are met with in which the sodium has not been mentioned.

The coloration caused by sodium is very intense; it overcomes every other color, and this is also the case with the red flame produced by the presence of potash.

To observe the coloration produced by potash, I prefer to make use of the solution of indigo in concentrated sulphuric acid prepared as described by Fresenius, but diluted only so far, that the white-heated platinum wire begins to be visible through it. Cobalt glass is good only when it contains a high percentage of oxide of cobalt.

The calcium combined with silicic acid never shows any coloration.

I discover five degrees in the yellow flame of sodium:

1. The yellow flame is very small. It is obtained by introducing into the flame the anorthite of Monte Somma. It corresponds to one or less per cent of Na_2O in the feldspar.

2. The yellow flame is a little larger. It is shown by the potash feldspar called valencianite from Mexico. It indicates 1–2 per cent of Na_2O (in the analysis made by Plattner only the potash is mentioned).

3. The yellow coloration is stronger. Observable in the bytownite, found in the gabbro of Volpersdorf (Silesia), 2–4 per cent, Na_2O.

4. The yellow coloration is intense. Shown by the potash feldspar called microcline from Fredericksvarn. 4–8 per cent, Na_2O.

5. The most intense yellow flame, oligoclase, St. Gothard, 8–16 per cent, Na_2O.

The red coloration produced by potash is very weak when the feldspar is exposed to the high temperatures by itself, and becomes visible only in such feldspars which contain more than four per cent of it. Now all soda and lime feldspars contain less than four per cent K_2O; therefore, if a red coloration is visible in the first two experiments, the feldspar is a potash feldspar of a greater or less percentage, and accordingly I discern three degrees.

1. Scarcely visible, doubtful. Loxoclase, Hammond, 4–10 per cent, K_2O.

2. Distinctly visible. Sanidin, Perlenhardt, Rhine, 10–13 per cent, K_2O.

3. Very distinctly visible. Adularia, St. Gothard; leucite; 13–20 per cent, K_2O.

In order to be quite certain as to the potash, a third experiment is made, by adding gypsum to the feldspar, by which the alkalies are converted into sulphates, and thus rendered volatile in such a manner that even a very small part of one per cent becomes very distinctly observable. As regards the sodium, the five degrees remain the same as in the first two experiments, but for the potash four degrees can be discerned, of which the first two relate to the plagioclase, the last two to the orthoclase.

1. Very weak and visible only for some seconds. Anorthite of Monte Somma, 0–1 per cent, K_2O.

2. Weak but very distinct and lasts for two minutes. Andesine from the trachyte of Déva (Transylvania), 1–4 per cent, K_2O.

3. Strong red flame, clearly visible for two minutes. Sanidin of Perlenhardt (Rhine), 4–13 per cent, K_2O.

4. The red flame is very strong and permanent for more than two minutes. Adularia of St. Gothard, or leucite of Albano, 13–22 per cent, K_2O.

Fusibility and the quality of the fused matter.

It has been mentioned that the degree of fusibility, as regards the feldspars, varies from zero to five degrees; but besides this character, there is a remarkable difference in the quality of the fused matter. In all cases, when a feldspar is fusible, it assumes this state slowly without intumescence; if this phenomenon is observable, for instance as is the case in many zeolites, the substance is not a feldspar.

The fused matter may be a pellucid or transparent glass, or an opaque enamel. The glass may contain bubbles or not; the enamel is sometimes gray, sometimes white and may contain bubbles. The bubbles are situated for the most part inside, and the surface of the globule is smooth, but many of the potash feldspars exhibit the peculiar property of having also outside bubbles; this is the case especially with the orthoclases of granites and other old rocks, while the glassy potash feldspars never show this character. Whenever the microscope gives us notice of the presence of microcline, the outside bubbles are never wanting.

Practical course of the determination.

The Bunsen burner, and two holders on each side of it to the right and left, are placed on a thick piece of glass, in order to secure a smooth motion. Two feldspars may be determined at once, or one feldspar, and on the other holder may be put a typical feldspar for comparison. If the grain will not adhere to the moistened ring of the platinum wire, the ring may be too large or too small for the size of the mineral fragment. Sometimes the platinum takes up no water; in that case I place the grain on the ring by means of a finely pointed pencil, or any other non-elastic matter.

The mineral fragment must be looked at with a Coddington lens in order to observe the slightest change in the consecutive experiments.

There are three experiments to be made, the first two with the mineral alone, but introduced at different temperatures for one minute; the third is made in the high temperature, but with addition of gypsum, and exposing to this high temperature for two minutes. The results observed are inscribed in blank forms, which must have columns for every observable phenomenon, so that after the three experiments we get a group of characters, which enables us to give the name of the feldspar, supposing that it was in a normal condition, or at least not far from it.

The next table shows the order of succession of the three experiments and their numerical results, not only for the ten series of feldspars, but also for some feldspathic minerals found in volcanic rocks, especially of newer date. At the end, two common minerals, the amphibole and augite of one special locality in Hungary, are exhibited in their somewhat unusual behavior; both those minerals being usually fusible 4–5.

TABLE

SHOWING THE ORDER OF SUCCESSION OF THE FLAME-EXPERIMENTS AND THEIR NUMERICAL RESULTS FOR THE FELDSPARS, AND SOME ASSOCIATED MINERALS.

FELDSPARS.	I Experiment. 5 mm. 1 minute.				II Exper. Region of fusion. 1 min.				III Exp. with gypsum.		Flame reaction of the HCl solution.
	Na 1-5	K 1-3	Degree of fusion, 1-5	Quality of the fusion.	Na 1-5	K 1-3	Degree of fusion, 1-5	Quality of the fusion.	Na 1-5	K 1-4	
1 Adularia	1-2	3	1-2	Glassy, sometimes bubbles outside.	2-3	3	3	Often bubbles outside.	2-3	4	Ca 0, Na 1-2, K 0.
2 Amazonite	2-3	3-2	2-3	"	2-3	3-2	3-4	"	3	3-4	Ca 0, Na 1-2, K 0.
3 Perthite	3-4	2	3	"	3-4	2	4	"	3-4	3	Ca 0, Na 1-2, K 0.
4 Loxoclase	4	1	3-4	"	4	1	4-5	"	4	2-3	Ca 0, Na 1-2, K 0.
5 Albite	5	0	3-4	Glassy, contain bubbles.	5	0	4-5	Bubbles inside.	5	0-1	Ca 0, Na 1-2, K 0.
6 Oligoclase	5-4	0	3-4	Glassy, or enamel-like.	5-4	0	4-5	"	5-4	1-2	Ca 0, Na 2, K 0.
7 Andesine	4-3	0	2-4	Glassy, transparent.	4-3	0	3-5	"	4-3	1-2	Ca 0-1, Na 2-3, K 0-2.
8 Labradorite	3-2	0	1-2	" "	3-2	0	2-3	Enamel-like.	3	1-2	Ca 1-2, Na 2-3, K 0-1.
9 Bytownite	3-2	0	1	Glassy, whitish.	3-2	0	1-2	Glassy, with some bub's.	3-2	0-1	Ca 2-3, Na 1-3, K 0-1.
10 Anorthite	2-1	0	0	Nearly no change.	2-1	0	0-1	Glassy, without bubbles.	3-2	0-1	Ca 2-4, Na 1-3, K 0-1.
Leucite	2-3	3	1	Glassy, translucent.	2-3	3	2	Glassy, no bubbles.	2-3	4	Ca 0, Na 2-3, K 4.
Nephelite	5	1-3	2-3	Glassy, enamel-like.	5	2-3	3-4	Glassy, with rare bubbles outside.	5	3-4	Ca 0, Na 5, K 3-4.
Nosite	5	1	1	A little glassy, grayish.	5	1-2	2	Partly translucent.	5	2-3	Ca 0, Na 5, K 3.
Hauynite	5	1-2	1	Nearly no change.	5	1-3	2-3	Partly glassy.	5	2-3	Ca 0, Na 5, K 1-3.
Sodalite	5	0-3	1-3	Glassy, whitish.	5	0-3	3-4	Glassy, bubbles.	4-5	0-3	Ca 0-1, Na 5, K 1-3.
Amphibole	2	0	1-2	Black, no lustre.	2	0	4	Black, shining.	3	1	Ca 0, Na 1-2, K 0.
Trachyte, Drégely var Augite	1	0	0	No change.	1-2	0	1-2	Black.	2	0-1	Ca 0, Na 1, K 0.
Trachyte, Drégely var											

In the *first* experiment, without the chimney, the mineral is introduced at the lower temperature (5 mm.) for one minute. The coloration caused by the sodium is estimated, and then immediately the observation with the blue indigo-fluid is taken in order that it may be completed at the end of one minute. After this time the holders are removed and the grains observed with a lens in order to give a number to the degree of fusibility and to its quality. All results are noted in their respective columns on the form.

Second experiment. The chimney is put on, the platinum wire is placed at such a height, that it may reach the region where the highest temperature is obtainable. All the observations are the same as in the first experiment, except that for the degree of fusibility as well as for its quality which is now conclusive.

Third Experiment. The same grain is exposed to the same temperature again, but after having been moistened with distilled water, and brought in contact with powdered gypsum, in order to cover it with it on all sides, and in that state it is introduced into the flame for two minutes. Now only the degrees for sodium and potash are noted.

General remarks for the three kinds of feldspars. For the *potash feldspars* the presence of this protoxide element in a higher percentage is the most essential character, and this is determined by the first two experiments, the potash feldspars showing alone the red flame and none of the plagioclastic, soda, or lime feldspars. The first experiment will therefore determine whether the feldspar is an orthoclase or a plagioclase.

The potash feldspars are not acted upon by concentrated hydrochloric acid; there is only a very small quantity of sodium extracted, so that the solution introduced into the flame produces a feeble yellow tint.

For the *soda feldspars* the great quantity of sodium is characteristic, and visible at the very moment when the mineral is introduced into the flame. At the same time the red flame must be absent. In the third experiment even the small quantity of potash contained in the soda feldspars becomes visible. The oligoclases contain always more or less potash, but among the albites there also occur specimens without potash.

The oligoclase is very commonly found together with the orthoclase in the same rock, and I have observed that whenever

the orthoclase is poor in potash (loxoclase) the oligoclase contains also very little of it; but with the amazonite or perthite a variety of oligoclase is always associated, in which the percentage of potash is correspondingly higher.

The soda feldspars are not acted upon by concentrated hydrochloric acid; the solution gives only a slight yellow coloration to the flame.

The *lime feldspars* are characterized by the gradual intensity of the yellow flame, the absence of the red flame, and by the lower degree of their fusibility. The degree of fusibility was not an essential character of the potash and soda feldspars, but it becomes very important in the case of the lime feldspars, the four series being, according to their increasing basicity, less and less possible.

The degree of fusibility for the						andesine	is 5–3
"	"	"	"	"	"	labradorite	" 3–2
"	"	"	"	"	"	bytownite	" 2–1
"	"	"	"	"	"	anorthite	" 1–0

The quality of the fused material shows also some slight peculiarities. The anorthite is not fusible, or fusible only at the points, the fused part is glassy, and contains no bubbles; the bytownite is similar in behavior but with a somewhat more advanced degree of fusibility; the fused labradorite is in most cases a whitish or grayish enamel, not unlike to some chalcedony; the andesine shows this character only in the first experiment, but in the second it gives a transparent or a limpid globule with bubbles inside, like the oligoclase.

It must be remembered, that while for the highest and lowest degrees of fusibility the size of the grain has no essential influence, if we take a fragment of quartz only the tenth part of the size required by the method, it will show the infusibility in the same manner, and on the contrary, if we take of borax ten times as much, it will melt nearly as easily as the standard size; but it is quite different with the middle and neighboring degrees. If the grain of labradorite is somewhat smaller, it shows the fusibility of andesine; in that case the experiment must be repeated with a somewhat larger fragment: if it fuses, it is andesine, if not wholly fusible, it is labradorite.

The concentrated hydrochloric acid exerts very little action when left in contact with the andesine, but the labradorite is partly decomposed, the bytownite still more, and much more the anorthite, the protoxide elements being accordingly converted into chlorides, which are very effective in coloring the flame.

The flame experiments with the hydrochloric solution.

The application of this well known principle is carried on in the following manner: I put in a short glass tube (height about 2 cm., diameter 5–6 mm.) some fine fragments of the mineral, and add only as much of the concentrated hydrochloric acid, as is required to cover it completely. The glass tube covered with wax is left for twenty-four hours; on the next day, at the same hour, I take out a drop by plunging in a thick platinum wire (like horse hair), which ends in double spiral rings. While the thin platinum wire, which we have employed for the flame reactions with the solid matter, would produce an instantaneous phenomenon, of which, I dare say, no use could be made, the platinum wire of the thickness mentioned will separate the chlorides according to their volatility. I introduce the drop into the base of the flame on the front side, and the following phenomena are to be observed. The first is the yellowish red coloration caused by the calcium chloride, but mixed with the yellow of sodium. To be certain that lime is present even in small quantities, I use a direct-vision spectroscope which shows the red and orange line very distinctly even for a trace of calcium. At the next moment the great bulk of the yellow coloration makes its appearance, and when arrived at its culmination, the red flame of the potash chloride may be observed through the indigo solution. The last of the usual colorations is that caused by lithium, visible through the spectroscope.

The degrees are marked about in the same manner as for the experiments in the dry way (see the table).

The platinum wire must of course be tried first alone for the coloration of the flame, and then moistened with the same acid, which is used for the decomposition of the mineral powder.

On the accompanying table we find the leucite as another potash mineral, having similar characters as the adularia in the dry experiments; but if we consider the flame reaction of the hydro-

chloric solution, then the great difference between the two will be evident. The same is the case with nepheline, nosite, sodalite or soda minerals; we find in the columns of the hydrochloric solution the greatest quantity of sodium, together with a high degree of potash, while the soda feldspars color with their hydrochloric solution the flame for soda very feebly, and for potash not at all.

For finding the calcium in such compounds, which are not acted upon by hydrochloric acid, for instance to make a distinction between oligoclase and andesine, I make use of the microchemical method of Boricky, by attacking the feldspar by hydrosilicofluoric acid, which shows the peculiar forms of calcium-silico fluoride in the andesine in larger quantity, and very little or none in the evaporated drop of the oligoclase solution.

Concluding remarks.

The method of determining the feldspars can be employed also for other classes of minerals with equally good results, but of course the alkalies giving the coloration, for minerals not containing alkalies, the result will be only the determination of fusibility. For the zeolites it gives some groups not only according to their volatile constituents coloring the flame, but also for the quality of the fused material, some being fusible with intumescence, some without it. It is very important also for the great class of the bisilicates (anphibole, augite, diallage, hypersthenite, enstatite, etc.), and it shows that some of them contain in more cases potash or sodium, or both of them, than has hitherto been supposed. There is a peculiar class of silicates however, the micaceous minerals, for which the result in respect of the quantitative indications is not the same as for the feldspars. The coloration caused by the potash never corresponds with its percentage, it is always less. For this class of minerals I make use of the method of Boricky.

The results obtained for the feldspars will give preliminary indications for their chemical analysis, knowing approximately the limits for the sodium and potassium; the limits for the other elements are given too. But its great importance in the lithological study of the crystalline rocks may be inferred by the fact, that it

is the unanimous conviction with petrographers, that every kind of a rock mass can be best determined by the mineral association found in it, and of all minerals it is the kind of feldspar which is the most important. There was a time, when we have been contented to say feldspar generally; then came a time when we said orthoclase and plagioclase, but now we know that this is not enough; we must discern among the plagioclases according to their basicity at least an oligoclase-andesine, a labradorite and a bytownite-anorthite. When we take a certain cycle of a well defined eruption, for instance, the eruption of the trachytic family, we find a correlative between the feldspar and the relative age: the trachyte with orthoclase-oligoclase, having the most acid constitution, was the first product of eruption, and the trachyte containing bytownite-anorthite and correspondingly having the most basic constitution, was the product of the last eruption, while the labradorite-trachytes are to be found between these two extremes. And that is the case for the cycles of other eruptions too, as far as some instances indicate it.

In the normal state of a rock it is the whole mineral association which must be taken into consideration, but in many cases the dark colored minerals are entirely changed, and only the feldspar to be found chemically in its normal state; then the determination of the feldspar is the only possible way of saying something of the lithological and chronological character of the rock in question, and the flame experiments furnish us with the result required in a short time and with sufficient exactness for the purpose aimed at.[1]

[1] Eine neue Methode die Feldspathe auch in Gesteinen zu bestimmen, von Dr. J. Szabó. Budapest, 1876. Berlin, Friedländer.

Elemente einer neuen chemisch mikroskopischen Mineral und Gesteinsanalyse, von Dr. E. Boricky. Prag, 1877.

APPLICATION OF ORGANIC ACIDS TO THE EXAMINATION OF MINERALS. *Third Paper.*

By H. CARRINGTON BOLTON, of Hartford, Conn.

37. IN the two papers previously published under the same title[1] we have shown that citric acid has a power of decomposing minerals little less than that possessed by hydrochloric acid, and that the organic acid can be advantageously employed in field-work for assistance in determining species. In the second of these papers we stated that the results obtained by the *prolonged* action of citric acid on minerals differed greatly from those recorded in the accompanying tables, and gave notice of our intention to return to the subject at a subsequent period. We have continued our researches and gained a few additional facts which we deem of sufficient interest to warrant a third or supplementary paper on the subject.

The results given in the tables (here reproduced for convenience) were obtained by acting on the minerals in fine powder: first, in the cold for a few hours, and secondly, at a boiling heat for a few minutes. We find, however, that many species which resist the brief action of a boiling concentrated solution of citric acid are more or less completely decomposed by prolonged contact with the same solution at the ordinary temperature of the work-room, say 60° to 70° Fahr. But a small number of the two hundred minerals embraced in the tables were submitted to the prolonged action of the acid solution, and those tested were selected as representative of groups, sulphides, oxides, silicates, etc. The progress of decomposition was noted at various periods from a few days to more than two years. A solution of citric acid concentrated in the cold was used in all the experiments.

38. *Sulphides.* Chalcocite showed signs of decomposition at the end of ten days, and at the expiration of several months yielded a partial solution of a pure green color.

Ullmannite showed signs of decomposing in two days, and afforded in a month a very dark colored solution.

Arsenopyrite was attacked in a few days. Pyrite showed decided evidence of decomposition in eight days; one month later the solution acquired a reddish yellow color and reacted for iron and sulphuric acid.

[1] Annals N. Y. Acad. Sciences. Vol. I, p. 1, 1877, and Vol. II, p. 1, 1880.

Chalcopyrite acts similarly, the solution being of a greenish yellow hue and reacting for copper and sulphuric acid. One gramme of the mineral lost about eleven per cent of its weight after fourteen months' contact with the acid solution. In general quantitative experiments were not conducted, for it soon became clear that the amount of decomposition was proportionate to the time and would require more attention than the results to be gained warranted.

Smaltite was decidedly attacked after eight days — the presence of sulphuric acid in the colored solution was noted. Tetrahedrite was strongly attacked in about four months. Cinnabar, orpiment, argentite and pyrargyrite resisted the acid solution altogether.

39. *Oxides.* The experiments recorded in this brief supplement were made with only those minerals which resisted the acid solution on boiling, and since it was obviously useless to test such bodies as quartz, corundum, rutile, etc., only the three oxides of iron remained of this class for examination.

Magnetite and limonite were strongly attacked in eight days; hematite yielded much more slowly, showing decided decomposition after several months.

Exposure to sunlight, as might be expected, diminished the intensity of the colored iron solutions.

40. *Silicates.* Of the silicates examined datolite is by far the most quickly decomposed by the acid solution; after twenty-four hours it yields gelatinous silica. Datolite should probably be transferred from column H of the tables to column D.

Hornblende, pyroxene, almandite, epidote, vesuvianite and serpentine were decidedly decomposed in eight days; after one month hornblende, almandite and epidote yielded colored solutions and slimy silica, and serpentine gave gelatinous silica. After fourteen months contact with the acid solution (occasionally agitated) the hornblende and pyroxene were very completely decomposed, the solutions having a dark brown color and full of floating silica. Actinolite seems to be less easily decomposed than common black hornblende. The serpentine after fourteen months yielded a dry gelatinous mass.

The feldspars are very unequally attacked under similar conditions. Labradorite yields the easiest, decomposition being noticed after eight days and slimy silica forming in the course of some months. Orthoclase and oligoclase showed marked signs of decomposition after eight months, but albite is doubtful. Tourmaline

and staurolite yielded to the acid after four or five months, the latter forming a solution colored by iron and slimy silica. Talc and kyanite appear to resist the action of citric acid.

Muscovite and biotite yield very slowly, the latter showing signs of decomposition the sooner. After two years' subjection to the acid solution very minute scales of mica separate, and some slimy silica appears in the slightly colored liquid.

41. We have detailed these simple observations somewhat minutely because we believe they have an important bearing on geological disintegration and erosion. At the Saratoga meeting of the A. A. A. S., my friend Dr. Alexis A. Julien presented an exhaustive summary and analysis of all known observations, concerning the geological action of the humus acids[2]; in this paper he notes the negative character of the results I had obtained with a number of minerals, *e. g.*, pyrite, augite, hornblende, the feldspars, micas, etc., and says: "but the nature and strength of the solvent employed, and the brief duration of each trial seem at present insufficient to justify generalizations, notwithstanding the suggestiveness of certain results." My subsequent observations have confirmed his views. Generalization is still of doubtful alue, yet we are tempted to make the venture. In the following table we have arranged the chief rock-forming minerals, with a

TABLE

SHOWING APPROXIMATE RELATIVE DISINTEGRATION OF ROCK-FORMING (AND ASSOCIATED) MINERALS BY CITRIC ACID IN SOLUTION.

Quickly decomposed.	*Slowly decomposed.*	*Very slowly decomposed.*	*Not decomposed.*
Carbonates, Phosphates, Prochlorite, Chrysolite, Nephelite.	Serpentine, Pyroxene, Hornblende, Labradorite, Garnet, Epidote, Vesuvianite, Pyrite, Limonite, Magnetite, Gypsum (?).	Orthoclase, Oligoclase, Albite (?), Biotite, Muscovite, Tourmaline, Staurolite, Hematite.	Quartz, Corundum, Spinel, Beryl, Fluorite, Barite, Talc (?), Kyanite (?).

few of their associates, under four heads indicating their relative disintegration by citric acid; the position of each mineral is of course only approximate, and it is not intended to indicate that all the species in one column are equal in degree of their decomposition. The application of this table to the consideration of

[2] Proceedings Am. Assoc. Adv. Sci., Saratoga Meeting, Vol. 28, p. 311, 1879.

geological phenomena is a subject which I do not venture to discuss; if we have a right to assume that the disintegrating effect of the acids of humus differs from that of the other organic acids in degree rather than in kind, the Table may prove a small contribution to chemical geology.

TABLES

SHOWING THE BEHAVIOR OF CERTAIN MINERALS WITH CITRIC ACID, ALONE AND WITH REAGENTS.

I.

DECOMPOSED (IN FINE POWDER) BY A SATURATED SOLUTION OF CITRIC ACID.

1. IN THE COLD.

A. *Without evolution of gas.*	B. *With liberation of CO_2.*	C. *With liberation of H_2S.*	D. *With separation of $Si\,O_2$.*
Clausthalite, Leucopyrite, Atacamite, Brucite, Gummite, Pyromorphite,* Mimetite, Triphylite, Triplite, Vivianite,! Libethenite,! Olivenite,! Pseudomalachite, Wavellite, Pharmacosiderite,! Torbernite, Autunite, Ulexite,! Cryptomorphite,! Anglesite. Brochantite.!	Calcite,! Dolomite,* Gurhofite,! Ankerite,* Rhodochrosite,* Smithsonite,* Aragonite,! Witherite,! Strontianite,! Barytocalcite,! Cerussite,! Malachite,! Azurite.*	Stibnite, Galenite, Alabandite,! Sphalerite, Pyrrhotite.	Wollastonite, Rhodonite,! Chrysolite, Willemite,!‡ Nephelite, Lapis lazuli, Chondrodite, Pectolite,!‡ Laumontite,!‡ Chrysocolla,! Calamine.!‡ Apophyllite, Thomsonite.! Natrolite,!‡ Mesolite,! Analcite, Chabazite, Herschelite,‡ Stilbite, Deweylite, Prochlorite.

2. ON BOILING.

E. *Without evolution of gas.*	F. *With liberation of CO_2.*	G. *With liberation of H_2S.*	H. *With separation of SiO_2.*
Cuprite,! Zincite, Melaconite, Goethite,* Limonite,* Allanite, (?) Apatite,* Wolframite,* Wulfenite, Crocoite, Gypsum.*	Hausmannite,† Pyrolusite,!† Manganite,† Psilomelane,!† Wad,!† Magnesite,! Siderite.!	Bornite, Jamesonite,* Bournonite.* Boulangerite, Kermesite.	Tephroite,‡ Ilvaite, Phlogopite,* Datolite,!‡ Prehnite,* Heulandite, Serpentine, Chrysotile, Retinalite, Bastite, Genthite, Gieseckite,* Jefferisite, Masonite.*
and those in A.	*and those in B.*	*and those in C.*	*and those in D.*

II.

DECOMPOSED BY A BOILING SOLUTION OF CITRIC ACID MIXED

I. WITH SODIUM NITRATE.		K. WITH POTASSIUM IODIDE.	
Silver, Mercury, Copper, Arsenic, Antimony, Bismuth, Sulphur,* Bismuthinite, Domeykite,! Argentite, Hessite, Chalcocite,! Tiemannite,! Millerite,! Niccolite,! Pyrite,! Chalcopyrite,! Linnaeite, Smaltite,!	Cobaltite,! Ullmannite,! Marcasite.! Arsenopyrite,! Nagyagite, Covellite,! Berthierite,! Pyrargyrite, Tetrahedrite,! Tennantite,! Stephanite, Polybasite,! Enargite,! Uraninite,! Hübnerite. *and those in C and G.*	Realgar,* Orpiment,* Cinnabar,! Hematite,* Menaccanite,* Washingtonite,* Magnetite,* Franklinite, Braunite, Enstatite, Hypersthene, Augite, Spodumene,* Hornblende,* Actinolite,* Pargasite,*	Olivine. Almandite, Pyrope. Colophonite, Epidote. *and most of those in A, B, C, D, E, F, G, H, and I.*

III.

L. MINERALS NOT DECOMPOSED BY THE ABOVE REAGENTS.

Graphite, Molybdenite, Proustite, Fluorite, Cryolite, Corundum, Diopside, Petalite, Asbestus, Beryl, Zircon, Vesuvianite, Zoisite,	Iolite, Biotite, Muscovite, Lepidolite, Wernerite, Leucite, Andalusite, Fibrolite, Kyanite, Topaz, Titanite, Staurolite, Bowenite,	Talc, Kaolin, Ripidolite, Columbite, Samarskite, Spinel. Chromite, Chrysoberyl, Cassiterite, Rutile, Quartz, Hyalite, Anorthite,	Labradorite, Oligoclase, Albite, Orthoclase, Tourmaline, Scheelite, Barite, Celestite, Anhydrite. (Two hundred species.)

N. B.—The gases evolved are examined with acetate of lead test paper, the solutions with appropriate reagents.

The kind and degree of action are indicated in the above Tables by the following signs:—

! Completely decomposed or dissolved.

* Feebly attacked.

† The CO_2 evolved is derived from the Citric Acid.

‡ Gelatinizes.

CARBON DIOXIDE AS A CONSTITUENT OF THE ATMOSPHERE. By ERNEST H. COOK, of Newton Villa, Southville, Bristol, England.

[ABSTRACT].

ALTHOUGH occurring in such a relatively small amount in the atmosphere, this substance has taken a leading part in effecting geological changes and in building up strata of immense thickness. We are apt to underrate its influence. When it is remembered that were it not for its presence in the atmosphere, no coal, very little limestone, probably no vegetable, and hence little animal life could exist, we at once see its importance.

In the present communication the author calculates the absolute amount of dioxide existing in the atmosphere, notices the sources whence the supply is derived, the amount yielded by each, and the causes at work abstracting dioxide from the air. On many of these points our knowledge is so unsatisfactory and fragmentary that the author hopes the attention of scientific men may be called to a subject of such importance to the geologist, meteorologist and chemist.

Amount of Carbon Dioxide in the Atmosphere.

Two things are necessary in order to determine this, viz.: the capacity of the atmosphere and the percentage of CO_2 contained by it. Taking the lengths of the polar and equatorial diameters of the earth and of the height of the homogeneous atmosphere, it is easy to calculate the capacity of the atmosphere. This is found to be equal to 591,647,337 cubic miles, or 2,439,987,200,000 millions of kilolitres. With regard to the average amount of dioxide contained in air, experimenters differ. The older chemists Dumas and Boussingault, Loëwy and Saussure, have published results which yield a mean of 4 volumes in 10,000 of air, or 4½ parts in 10,000 by weight. On the other hand, more recent experiments conducted by Thorpe, Fittbogen and Hasselbarth, Farsky, Reiset and others, indicate that 3 volumes in 10,000 of air is the correct number.

Assuming the first of these numbers to be the true one, we find that the amount of CO_2 in the atmosphere is very nearly 1,913,686,000 millions of kilogrammes, while if we take the more

recent determinations, we find 1,435,265,000 millions of kilogrammes. These numbers differ considerably from those previously given. Thus Dumas and Boussingault say[1] that the air is equal in weight to 581,000 cubes of copper, each having a side of 1 kilometre—a number which is 40 per cent higher than the greater of the two numbers stated above. Again Roscoe and Schorlemmer[2] state that "the amount of CO_2 in the atmosphere reaches to upwards of 3,000 billions of kilogrammes" which is again 33 per cent above the same number.

Sources whence the air derives its Carbon Dioxide.

These may be grouped as follows:

(1). Combination of carbonaceous bodies.

(2). Respiration of animals.

(3). Decomposition of animal and vegetable substances.

(4). Volcanoes and other subterranean supplies. Under the first heading is included the amount produced by the burning of coal, wood, peat, etc. Taking the statistics which have recently been issued with regard to the amount of coal raised in the world, it will be found that for the last three years at least 280,000,000 of tons have been raised annually. Assuming 75 per cent of this consists of pure carbon, and allowing a further 10 per cent for the carbonaceous material thrown away with the ash, we leave 182,000,000 of tons which are annually converted into carbon dioxide. Thus there is daily added to the atmosphere from this source 1,800,000,000 of kilogrammes of dioxide. By the combustion of wood, peat, oil, etc., we may assume one-third more to be added, thus making a total of 2,400,000,000 of kilogrammes per day derived from the combustion of carbonaceous substances.

In the case of the respiration of animals we can only form an approximate estimate. The population of the world is at present about 1,500,000,000, and experiment shows that an average individual produces about one kilogramme of CO_2 in a day of twenty-four hours. Thus the human race in respiring adds daily to the air 1,500,000,000 kilogrammes. From the lower animals we may consider twice this amount to be derived; thus from the whole

[1] Ann. Ch. Phys., iii, 257, 288.

[2] Treatise on Chemistry, Vol. I, p. 449.

animal kingdom about 4,500,000,000 of kilogrammes of dioxide are daily added to the atmosphere.

The decomposition of vegetable and animal substances must obviously yield a large quantity of dioxide; but practically nothing is known of the nature of the change or of the amount produced. In order to fix our ideas we may assume that this source yields the same quantity as is yielded by man, viz.: 1,500,000,000 kilogrammes.

The last source mentioned is by far the greatest. Not only do active volcanoes produce, but extinct and volcanic districts join in increasing the quantity. Considering the immense area covered by these districts and the immense quantity of gas evolved, we must conclude that by far the greatest amount of atmospheric dioxide has been obtained from this source. In fact, Poggendorff calculated that ten times as much is produced by volcanoes as by all the other sources put together. Allowing for the amounts previously given being larger than similar numbers used by Poggendorff, we may take it that 40,000,000,000 kilogrammes are daily given to the atmosphere from these subterranean sources.

Finally, if we add together these various amounts, we find that the air is daily receiving an addition of about 50,000,000,000 of kilogrammes of carbon dioxide. Dividing the absolute amount contained in the whole atmosphere by this number, we find *that the amount of carbon dioxide in the atmosphere would be double what it is at present in about one hundred years if there were no means of compensation.* Poggendorff, who made a similar estimate, gave three hundred eighty-six years as the time required to double the amount of CO_2 in the air. The difference is explained by the absolute amount of CO_2 being at that time considered greater than it really is, and also by the amount of carbonaceous matter burnt being greater now than in his time.

The disagreeable nature of the reflections called up by this last calculation disappear, however, when we come to consider the

Compensating Influences.

The known causes at work removing the dioxide may be considered under three heads, viz.:

(1). Fixation of carbon by growing plants.

(2). Removal of dioxide by zoophytes.

(3). Absorption of dioxide by inorganic chemical actions.

The first cause here mentioned is one which is essential to vegetable growth. In estimating its magnitude we are met by the want of reliable experimental data, making it almost impossible to arrive at any definite conclusion.

Certain experiments have shown that speaking approximately, a square metre of leaf will decompose one litre of CO_2 per hour in sunlight. The area of the land capable of bearing vegetation amounts to 49,400,000 square miles. Assuming the area of leaf borne by this land to be 50 per cent of this amount, we find that about 63,726,000,000,000 of square metres of leaf are hourly engaged in purifying the atmosphere. This decomposing effect will go on for about ten hours out of every twenty-four, but will be less active in the autumn and winter months than in spring and summer.

Taking all things into consideration (allowing 25 per cent for diminution in winter) we find that vegetation daily decomposes about 478,000,000,000 of kilolitres of CO_2, or about 900,000,000,000 of kilogrammes, an amount which is much greater than the amount produced from all sources. The magnitude of the action exerted by plants is surprising, but it must be remembered that during darkness plants expire CO_2. No account is taken of this action here, as no quantitative experiments seem to have been made upon it. The author thinks that this action goes on to a greater degree than is usually supposed.

The second great action going on in nature is effected by the interposition of animal life. It consists in the removal from sea-water of the carbon dioxide held by it in solution, by certain low forms of animal life, such, for example, as the coral-builders and the foraminifera. (Certain other forms, such as brachiopoda and lamellibranchiata also fix CO_2 as calcic carbonate; their effects are, however, comparatively insignificant.) The immense influence exerted by these organisms is evident when we look at the vast masses of limestone occurring in geological formations of all ages which have an exclusively organic origin. Nor is this influence confined to the past. Recent deep-sea soundings have revealed the fact that foraminiferal life still flourishes in the depths of the ocean, while we know that coral-reefs are still being constructed in the warmer seas. In short, this cause must be considered as the chief one which is active in removing dioxide, but want of data prevents our estimating its amount.

The third action going on in nature, effecting the purification of the air, is a purely inorganic one. Included under this head are such processes as the conversion of feldspar into kaolin, the decomposition of such silicates as hornblende, pyroxene, etc. The large deposits of kaolin and decomposed feldspar, which are met with in the earth, sufficiently prove the magnitude of this action. Thus "a weight of carbonic dioxide equal to more than twenty-one times that of our present atmosphere would be absorbed in the production from orthoclase of a layer of kaolin extending over the earth's surface with a thickness of 500 metres, an amount which evidently represents but a small proportion of the results of feldspathic decay in the sedimentary strata of the globe."[3] Evidently then here we have a cause which has an important influence in effecting the removal of atmospheric carbon dioxide. It is impossible, however, to estimate its amount.

General Conclusions.

It is evident that if the compensating influences are just equal in total effect to the productive ones, the amount of carbonic dioxide in the air remains constant, but an insufficiency of data prevents a definite answer being given to this. We know, however, that these processes have been going on for a very long time. In fact, there is already fixed in the earth an immense quantity of CO_2 equal to many hundreds of times the amount contained in the present atmosphere. This must have been derived from the atmosphere, but it is not necessary to assume that it *all* formed part, at one time, of the atmosphere. It must, however, be evident that at one time more CO_2 existed in the atmosphere than at present.[4] If this be so, the compensating influences must be more powerful than the producing ones, and the proportion of atmospheric dioxide must be diminishing.

[3] Dr. T. Sterry Hunt. *Chemical and Geological Relations of the Atmosphere.* American Journal of Science, May, 1880.

[4] Unless we accept Dr. Hunt's theory that space is filled with an atmosphere similar to our own, and that CO_2 fixed in the earth at one time formed part of this universal atmosphere.

A SIMPLE LABORATORY DEVICE. By ERNEST H. COOK, of Newton Villa, Southville, Bristol, England.

[ABSTRACT.]

IN this communication the author described an improvement in water and air ovens which he has employed with advantage in his own laboratory. This consists in applying the principle of the Bunsen water pump to these instruments. Thus instead of being closed boxes as is usually employed, a current of air is drawn through them and over the substance to be dried. With this simple addition, the time of drying is greatly reduced; and without any alteration of apparatus, substances can be completely dried at a low heat, and in fact at the ordinary temperature, by simply increasing the rapidity of the current of air traversing the chamber.

The same principle is sometimes usefully applied to small portable stink cupboards which stand upon the laboratory bench. The practical details are so simple and self-evident that a description is unnecessary.

THE ACTION OF WATER AT 100° C. ON THE β PHENYLTRIBROMPROPIONIC ACID. By LEONARD P. KINNICUTT and GEORGE M. PALMER, of Cambridge, Mass.

[ABSTRACT.]

THE action of boiling water on the α phenyltribrompropionic acid has been described by one of us in the American Chemical Journal for April. Since that time the study of the β acid has engaged our attention.

This acid is obtained by the action of bromine on monobromcinnamic acid, and is a crystalline solid soluble in alcohol, ether, bisulphide of carbon and chloroform. Its melting point is 151° C. and not 48° C. as given by Glaser (Ann. 143, P. 339), nor does it

first separate out as an oil. The acid obtained by Glaser was evidently an impure substance.

On boiling with water the acid is decomposed, a bibromstyrol, whose structure we have not as yet determined, and the α monobromcinnamic acid being formed; also a small quantity of a new acid, very soluble in water, melting at 184°, which on analysis gave amounts of carbon, hydrogen, and bromine, corresponding very closely to what would be obtained if the substance was a phenylbibromlactic acid.

The formation of the same α monobromcinnamic acid, from both the α and β phenyltribrompropionic acid is not easily explained. It is probably caused by a change of position of the atoms within the molecule at the moment of its formation.

We are at present engaged on a further study of the above reaction, together with the products formed by the action of chlorine on both the α and β monobromcinnamic acids. *Chemical Laboratory, Harvard University.*

The action of Baric Hydrate on Chlortribrompropionic Acid.

By C. F. Mabery and Ralph Wilson, of Cambridge, Mass.

[ABSTRACT.]

In a previous paper by H. C. Weber and one of us, it was stated that chlortribrompropionic acid was decomposed when heated with baric hydrate. The products of this decomposition were not sufficiently studied to enable us to characterize them with any degree of accuracy, although the formation, to some extent, of tribromethylen was observed. In repeating these experiments we find that chlortribrompropionic acid, when heated with two molecules of baric hydrate in aqueous solution, is almost completely decomposed into baric bromide, carbonic dioxide and tribromethylen. The latter product was absorbed by bromine water, the ex-

cess of bromine allowed to evaporate and then crystallized from hot alcohol.

The following analysis was made:

0.1556 product gave 0.3657 grms. Ag Br + Ag Cl, which corresponds to 93.30 per cent 4 Br + Cl. Calculated for $C_2 Br_4 Cl H$ 93.40 per cent.

This substance melted at 33° and was probably identical with monochlortetrabromethan, melting point, obtained by Wallach and Bischof (Berlin Berichte, 1878–751) from the decomposition of dichloracrylic acid. Since monochloracetylen obtained in this reaction must have the constitution C H=C Cl, the product obtained by us must have the hydrogen and chlorine atoms attached to adjacent carbon atoms. This would give for chlortribrompropionic acid the structure C Cl Br_2 C H Br COOH, and for chlorbromacrylic acid the structure C Cl Br C H COOH, which H. B. Hill has already inferred from the relations of the two disubstituted acrylic acids.

On Certain Substituted Acrylic and Propionic Acids. By C. F. Mabery, of Cambridge, Mass., and F. C. Robinson, of Brunswick, Me.

[ABSTRACT.]

By the action of bromine on the dibromacrylic acid first obtained by Fittig and Petri from dibromsuccinic acid, we have made a tetrabrompropionic acid which differs essentially in its properties from the tetrabrompropionic acid obtained by H. B. Hill, and one of us (C. F. M.), from the dibromacrylic acid derived from mucobromic.

We have studied the salts of this acid sufficiently to characterize it. When heated with baric hydrate the acid is decomposed, tribromethylen being the chief product of its decomposition. By the action of alcoholic potash hydrobromic acid is eliminated with the formation of tribromacrylic acid which melted at 117–118°.

We have also tried the action of bromine monochloride upon brompropiolic acid. Although the investigation is not yet com-

pleted we have reason to believe that the elements of bromine monochloride are taken up with the formation of dibromchloracrylic acid.

Bromine monochloride was made by the action of chlorine upon bromine at 0° in a chloroform solution.

On the Products of the Distillation of Wood at Low Temperatures. By C. F. Mabery, of Cambridge, Mass.

[ABSTRACT.]

This paper contains a description of the results which I have obtained in an examination of the volatile products of the dry distillation of wood. I have isolated acetic aldehyde and identified it by conversion into silver acetate which has been analyzed. Methyl formiate also was obtained in considerable quantity. It was saponified with plumbic hydrate and the plumbic formiate analyzed. I have found in the course of this investigation that methyl alcohol obtained by this process contains a much smaller per cent of aceton than is usually found in commercial wood spirit.

Preliminary Notice of a New Organic Base. By Albert R. Leeds, of Hoboken, N. J.

[ABSTRACT.]

When xylidine in alcoholic solution is treated with acrolein, the two bodies enter into combination to form acrolein-xylidine (its properties, analysis, etc., given), and water is eliminated. By sublimation, a limpid oil is obtained, which is cryptidine.

Description of its properties, chemical relationships, and certain of its salts.

Estimation of Dextrose in Solid Commercial Starch Sugar by loss of Rotatory Power on Solution. By Harvey W. Wiley and C. A. Crampton, of Lafayette, Ind.

[ABSTRACT.]

It is not generally recognized that the amorphous starch sugars of commerce have the same property of losing rotatory power on solution and heating as if in a crystalline state. This depends on their large content of dextrose. The principle of determination used by the authors depends on the well known fact that a freshly prepared solution of dextrose has a rotatory power twice as great as after standing a few hours or heating to the boiling point for a few minutes. The authors show by tables of analysis that the dextrose can be determined in this way with reasonable satisfaction in all starch sugars made by high conversion. Sugars, however, which still have a large percentage of maltose and dextrine will not permit of their dextrose being determined in this way. This probably arises from the fact announced by Meisl that maltose solutions increase in rotatory power in the same circumstances in which dextrose solutions decrease. The authors exhibit curves showing the rate of fall in rotatory power of dextrose solutions.

Direct Estimation of Dextrose, Dextrine and Maltose in Commercial Amylose (Starch Sugar). By Harvey W. Wiley, of Lafayette, Ind.

[ABSTRACT.]

The process is founded on a method of double reduction and polarization proposed by me at the Cincinnati meeting and since that time practically worked out.

The specific rotatory power and percentage of reducing matter of the sample are first determined. In another portion of the sample the whole of the reducing matter is destroyed by a slight

excess of mercuric cyanide and the residue, after neutralization with hydrochloric acid repolarized in a tube two and a half times longer than the first and the number thus obtained multiplied by two. This gives the specific rotatory power due to *dextrine* alone, from which the percentage of dextrine present is easily calculated from formulæ given in my paper at the Boston meeting. Having determined the dextrine, the maltose and dextrose are calculated from formulæ given in the paper.

The paper contains tables of analyses showing the results obtained by the direct method compared with those obtained by best approved methods heretofore known.

ON THE CONSTITUTION OF BENZOLE. By L. W. ANDREWS, of Brooklyn, N. Y.

[ABSTRACT.]

CLAUS maintains that Hübner's benzole formula is the one which best accords with the known facts of isomerism in the aromatic series.

This position is untenable, because the formula fails to explain the existence of three isomeric forms of bisubstitution products of benzole, unless on the basis of a method of interpretation which cannot consistently be carried out in all cases. Several considerations show that according to this formula the compound 1–2 should be identical with 1–4. These points are mutually interchangeable without rupture of the bonds or change in their distribution.

This fact is an infallible criterion of the identity or non-identity of the bonds 1–2 and 1–4. A model is shown to render apparent to the eye the peculiarities and imperfections of the "spider web" formula of *Hubner* and *Claus*.

Some Tea Analyses, with Special Reference to the Determination of Caffeine.

By W. Hodgson Ellis, of Toronto, Canada.

[ABSTRACT.]

The variation in the percentage of caffeine in tea leaves, reported by various chemists, is considerable, a circumstance due to the various methods of analysis.

After a number of trials of different methods, I have adopted the following process which gives in my hands the best results and which I have now used for several years.

1. The powdered tea is exhausted with boiling water under an inverted condenser.

2. The filtered solution is evaporated to dryness on the water bath with magnesia.

3. The residue is exhausted with absolute alcohol, and the filtered solution mixed with sand and evaporated on the water bath.

4. The residue is exhausted with hot distilled water and the solution again evaporated.

5. The residue is taken up with ether and the ethereal solution evaporated in a tared beaker and weighed.

By this method the caffeine is obtained colorless, and well crystallized.

Commercial tea as met with in Canada yields by this process from 1.5 to 3.5 per cent of caffeine.

In exhausted leaves, *i. e.*, leaves treated with boiling water, as in the ordinary method of making tea, the caffeine is nearly all removed, as in the following examples of three Japan teas and their exhausted leaves.

	1		2		3	
	Tea	Exhausted leaves.	Tea	Exhausted leaves.	Tea	Exhausted leaves.
Caffeine.....	2.5	.6	2.4	.5	3.7	.9
Ash.....	7.2	4.1	6.8	4.0	6.5	3.9
Soluble ash....	3.8	.8	3.5	.8	3.4	.7
Silica.....	.9	.8	.7	.8	.5	.7
Extract.....	37.8	16.8	40.3	13.4	33.2	8.8

In the following analyses the caffeine was determined by this method; and the extract, by boiling the powdered tea under an inverted condenser with successive portions of water. The ash, soluble ash and silica were also determined in each case.

GREEN TEAS.

No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.	No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.
1	39.8	5.0	3.6	0.5	2.2	19	28.7	6.8	3.0	0.5	2.4
2	43.4	4.1	3.7	0.2	2.4	20	31.8	6.4	3.3	0.5	2.6
3	43.4	6.2	3.6	0.4	2.3	21	34.7	6.1	3.7	0.3	2.6
4	45.2	3.6	3.6	0.3	2.0	22	34.2	6.8	4.1	0.5	2.7
5	38.0	4.4	3.8	0.3	2.5	23	33.7	6.2	3.4	0.3	1.9
6	39.2	3.0	3.3	0.5	2.6	24	33.0	7.7	3.9	1.1	2.1
7	45.2	5.2	3.5	0.5	2.3	25	34.8	6.8	4.0	0.3	2.6
8	40.2	4.0	3.4	0.3	1.9	26	35.2	6.7	3.0	0.9	2.1
9	41.2	6.1	3.9	0.5	1.9	27	35.0	6.9	3.7	0.8	2.4
10	39.4	3.8	3.6	0.7	1.3	28	36.4	6.9	3.2	0.9	2.9
11	37.9	4.1	3.7	0.9	2.3	29	33.8	6.0	3.3	0.2	2.1
12	38.6	4.6	3.6	0.3	1.9	30	27.4	6.3	3.8	0.4	2.5
13	45.8	4.6	3.6	0.5	2.4	31	34.2	6.3	3.6	0.6	1.7
14	38.5	3.7	3.6	0.4	2.3	32	36.0	7.3	3.1	1.3	2.0
15	35.2	4.3	3.1	1.0	2.6	33	39.0	9.9	2.7	3.5	2.3
16	37.4	5.0	3.8	0.3	1.8	34	38.0	6.4	3.7	0.4	2.1
17	41.3	4.7	3.8	0.4	1.9	35	45.0	6.1	3.4	0.4	3.0
18	34.1	6.2	4.1	0.4	2.3						

BLACK TEAS.

No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.	No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.
1	27.8	6.3	3.1	0.8	2.4	6	38.0	5.6	3.4	0.2	2.6
2	29.8	6.4	3.6	0.6	2.4	7	37.6	5.8	3.5	0.2	1.9
3	31.1	5.7	3.5	0.4	3.7	8	37.2	6.0	3.9	0.6	2.3
4	31.1	6.8	2.9	1.0	2.3	9	31.0	6.5	3.5	0.7	2.3
5	36.5	6.4	3.8	0.4	2.6	10	27.2	6.3	2.9	0.8	2.3

MIXED OR NOT DISTINGUISHED.

No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.	No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.
1	29.1	5.7	3.6	.4	2.1	11	30.6	5.9	3.1	.4	3.0
2	35.6	6.9	3.8	.9	1.6	12	30.0	6.2	3.9	.1	2.3
3	26.5	5.9	3.7	.3	2.2	13	31.1	7.0	3.2	.1	2.6
4	20.2	6.8	3.0	.3	2.2	14	29.8	6.6	3.5	.4	2.2
5	26.2	5.7	3.2	.6	1.9	15	24.8	6.5	2.6	.8	1.7
6	38.2	6.2	4.0	.5	2.1	16	26.8	6.1	3.1	.5	2.1
7	35.4	6.2	3.5	.8	2.5	17	35.6	6.0	3.7	.3	2.0
8	37.2	5.6	3.7	.3	1.5	18	34.8	5.8	3.4	.2	2.6
9	23.8	6.5	3.5	.8	2.2	19	30.2	6.2	2.8	.5	2.0
10	29.2	5.4	3.0	.7	1.6	20	26.2	6.5	3.4	.2	1.8

The above analyses may be summarized as follows:—

35 GREEN TEAS.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	40.6	6.5	3.5	0.6	2.3
Maximum .	45.8	9.9	4.1	3.5	3.0
Minimum . .	28.7	5.5	2.7	0.2	1.3

10 BLACK TEAS.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	32.6	6.2	3.4	0.6	2.5
Maximum .	38.0	6.5	3.9	1.0	3.7
Minimum . .	27.2	5.6	2.9	0.2	1.9

20 TEAS MIXED OR NOT DISTINGUISHED.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	30.0	6.2	3.4	0.5	2.2
Maximum . .	38.2	7.0	4.0	0.9	3.0
Minimum . .	20.2	5.4	2.6	0.1	1.5

ANALYSIS OF THE WHOLE 65 SAMPLES.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	36.2	6.5	3.5	0.5	2.2
Maximum .	45.8	9.9	4.1	3.5	3.7
Minimum .	20.2	5.4	2.6	0.1	1.3

PEMBERTON'S METHOD FOR THE VOLUMETRIC DETERMINATION OF PHOSPHORIC ACID. By PROF. G. C. CALDWELL, of Ithaca, N. Y.

[ABSTRACT.]

MR. H. PEMBERTON has given[1] a method for the volumetric determination of phosphoric acid in fertilizers, by means of a standard solution of ammonium molybdate, which consists simply in adding this solution as long as a precipitate is produced, the final tests for further precipitation being made with small filtered portions of the solution under examination. The standard solution is prepared by dissolving 89.543 gms. of ordinary ammonium molybdate in 1000 cc. of water; each cubic centimetre of such a solution precipitates

[1] Journal Franklin Institute, March, 1882, p. 184.

3 milligrammes of phosphoric anhydride. The solution of the phosphate, first freed from organic matter and silica, is finally made up with as small an excess of acid as possible, and just neutralized with ammonia; 2 cc. of strong nitric acid and 10 gms. of ammonium nitrate are added, and the solution is heated to 60 deg., or above, preparatory to the addition of the molybdate; from the total quantity of standard solution required 0. 5 cc. is deducted for the excess necessary to neutralize the solvent action of the nitric acid.

With two modifications of the details as described by Mr. Pemberton, the process has satisfactorily sustained a careful trial in my laboratory. First, as to the preparation of the standard solution, the author found that the ordinary salt of the druggists was sufficiently pure to give the standard directly; but in order to clear away the slight turbidity which it usually leaves he added a little ammonia; it was however observed by Dr. S. B. Newbury, then analyst to the University experiment station, that in the case of the molybdate supplied to us, a notable degree of alkalinity was communicated to the solution by the necessary quantity of ammonia to clarify it; consequently the acidity produced by the 2 cc. of acid, added to the solution of the phosphate in the beginning, became so much reduced towards the end of the titration, that the results were unsatisfactory; in some instances the solution even became slightly alkaline before the precipitation was completed. Therefore, in order to secure perfect neutrality, the molybdate solution was filtered, after the addition of a few drops of ammonia at the most, and then standardized by a solution of phosphorus salt of known strength.

The second modification refers to the method of taking out the test portions to be tested for complete precipitation; even when this is done as described by the author, this method of determining phosphoric acid is unquestionably more expeditious than the usual gravimetric method with molybdate and magnesia mixture; but the necessity of filtering out each of these test portions in the ordinary manner through a filter is objectionable, and might deter many from using the method. For this filtration I used with great success a small suction filter, somewhat after Carmichael's idea, which could be plunged directly into the liquid, and through which a small portion of the same could be drawn at once into a small test tube in which the test for further precipitation was made.

The results of our trial of the method are set forth in the following table: three of the sets of determinations were made by three experienced chemists with solutions prepared by one of the number, each one being entirely ignorant of the results obtained by the others; the fourth set was obtained by another chemist with solutions of molybdate prepared and standardized by himself, and with his own solutions of the same phosphates that were examined by the others.

NO. OF PHOSPHATE.	I		II	III	IV
	Gravimetric.	Volumetric.	Volumetric.	Volumetric.	Volumetric.
237		11.18			
		11.20			
235		13.14	13.27		13.13
		13.14			13.18
					13.15
					13.12
259	12.47	12.33	12.39	12.44	12.34
	12.41	12.33		12.45	
				12.47	
				9.20	
261	9.15	9.26	9.08	9.05	9.34
	9.21	9.26		8.97	9.37
				8.97	
272	13.69	13.57	13.75	13.82	
	13.82	13.63		13.81	
		13.57		13.72	
		13.69			
		13.63			

From these results it appears that the method is capable of great accuracy, but, as in the case of No. 261, is liable to some source of error, which makes great care necessary in the preparation of the solution of the phosphate, according to the directions given by the author.

TITLES OF OTHER PAPERS READ IN SECTION C.

EXPERIMENTAL RESEARCHES ON THE SOURCES OF NITROGEN IN CROPS. By J. B. Lawes and J. W. Gilbert, of London, England.

OBSERVATIONS ON THE CONTAMINATION OF CITY WELLS. By R. B. Warder, of North Bend, O.

ON THE CAUSES WHICH RENDER FLOUR AND ORGANIC DUST EXPLOSIVE, WITH SUGGESTIONS FOR THE PREVENTION OF SUCH EXPLOSIONS. By Thomas W. Tobin, of Louisville, Ky.

SOME DERIVATIVES OF ISOPICRAMINIC ACID. By Charles W. Dabney, jr., of Raleigh, N. C.

A BENZOYL ANHYDRO ACID FROM R-METAMIDOSALICYLIC ACID. By Charles W. Dabney, jr., of Raleigh, N. C.

NOTES ON EFFECTS OF DIFFERENT SOILS UPON SOLUBLE PHOSPHATES. By Charles W. Dabney, jr., of Raleigh, N. C.

NOTE ON THE ABSORPTION SPECTRUM OF HUMIC ACID. By H. Carrington Bolton, of Hartford, Conn.

NOTES ON BONE OIL.[1] By Arthur H. Elliott and Fred. Sands, of New York, N. Y.

ON NITRO-SACCHAROSE.[2] By Arthur H. Elliot, of New York, N. Y.

TRANSMISSION OF GASES THROUGH LIQUIDS OF DIFFERENT DENSITIES. By C. G. Wheeler and F. Menzel, of Chicago, Ill.

THE SOLUTION AND LATE CRYSTALLIZATION OF GOLD HEATED WITH CHLOROHYDRIC ACID IN A SEALED TUBE. By Henry Carmichael, of Brunswick, Me.

REMARKS ON THE APPLICATION OF THE IRIDIUM KNIFE-EDGE TO ANALYTICAL BALANCES. By William Dudley, of Cincinnati, Ohio.

FIRE-DAMP INDICATORS. By Isidore Kitsee, of Cincinnati, Ohio.

NOTES ON THE ANALYSIS OF SUPERPHOSPHATES.[3] By THOMAS S. GLADDING, of New York, N. Y.

REPORT OF THE AGRICULTURAL CHEMISTS. By Chas. M. Stillwell, of New York, N. Y., Secretary.

[1] Published in full in the Journal of the American Chemical Society for Aug., 1882.

[2] Published in full in the School of Mines Quarterly for Nov., 1882.

[3] Published in full in the Am. Jour. of Chemistry, for 1883.

SECTION D.

MECHANICAL SCIENCE.

ADDRESS

BY

PROFESSOR W. P. TROWBRIDGE,

VICE PRESIDENT, SECTION D.

MEMBERS OF THE SECTION OF MECHANICAL SCIENCE:—

WHEN we look upon mechanical science as an abstract science, having for its foundation only a few simple axioms, dynamical laws, and geometrical theorems, and divest it of many of its complicated mathematical applications, we can hardly help regarding the science as complete.

The controversies and discussions which marked its growth, and which for centuries absorbed the attention of the scientific world, have ceased, and there is left to the present generation a clear, well defined science treating of the forces of nature and the movements which they produce, or are capable of producing, in the ordinary phenomena of force and motion.

Even the last great extension of this science in the development of the true theory of heat, which in its conception and importance was but a fit culmination of the labors of a long line of illustrious men, has now become universally accepted in all its significant features; and although some of the men are now living who were chiefly instrumental in establishing the purely mechanical nature of heat, and in bringing all its manifestations into harmony with mechanical laws, yet this result seems now to have been only the realization of anticipations long looked for, and long before predicted.

No such anticipations now lie before us to excite the zeal of the investigator. There are now no such controversies or oppor-

tunities for discovery to stimulate analytical investigation as marked the last and the preceding centuries. Whatever may be the nomenclature chosen, the forms used, or the classification of subjects, there is no longer any contention regarding the theorems established by Benedetti, Descartes, Leibnitz, Newton, D'Alembert, Bernoulli, and others; and more recently in our own generation, by Clausius, Rankine, Helmholz, and Sir William Thomson.

But while we inherit in all its completeness a science so comprehensive and exact, in its generalizations, as to embrace the movements of the bodies of the solar system on the one hand, and the infinitesimal motions of the molecules of bodies, producing the phenomena of heat, on the other, there is still an unlimited field open for experimental research.

As far as mechanical science is concerned it may be said that we live in an era of *applied science*,— an epoch which has already produced astounding and unexpected results, modifying and changing all the relations of civilized life.

Concerning abstract principles, no kind of knowledge is more widely diffused or universally accepted than that which relates to the subject we are considering. We live and move through the medium of appliances which are but the exemplifications of well known mechanical principles; and with the firm foundations of an exact science to build upon, there is an increasing activity of thought and action, directed, not to the discovery of new principles, but towards new methods, new applications, greater mechanical achievements.

To employ the language of mechanics, there exists at the present time an immense momentum of intellectual force, in this respect, which must either be properly directed to overcoming obstacles in the way of progress, or dissipated in costly failures.

Speculations as to what may be the results of this stored up mental energy, in future developments of the mechanic arts, are unprofitable; but well-directed efforts towards the determination of new applications, or the experimental verification of inductive demonstrations, are seldom without interest; and the more thorough and complete such investigations are made, the greater will be their subsequent value.

There is a wide range of applications of mechanical science to engineering, and in the arts, for which such experimental researches are now demanded.

The recent united appeal of the three principal engineering societies of this country, urging the continuance of appropriations by the United States Government for experiments on the strength and properties of the materials used in engineering structures, is evidence of the want that is felt of a better knowledge of this particular subject.

Among the forces of nature with which we have to deal, there is one class of which the laws have not been subjected to exact determination.

Whatever may be the nature of the forces of gravity and inertia, the laws governing their action are definitely determined; but of the molecular forces which resist the disruption of materials, and which govern their action under the influence of external forces, but little is definitely known; all that is known has been derived from special experiments on specific forms and qualities of materials, which, from the very nature of these materials, are not yet sufficient for general or universal applications. And when it is considered that not only the cost, but the absolute safety, of all engineering structures depends upon this uncertain knowledge, the importance of the question becomes evident. So little has been done in this country in many branches of this subject that reliance is placed almost wholly on the results of investigations made abroad—investigations which were never entirely applicable to the material found here; and which, on account of the rapid changes in the arts of manufacture and fabrication, have become to a certain extent obsolete, or of very uncertain value.

In relation to metallic products, it may be said that a new field of technical inquiry has sprung up, lying between the science of metallurgy and the profession of engineering.

It can hardly be said that the metallurgist is required to answer all questions relating to the properties of the metals in the forms in which they reach the engineer. These properties are determined or modified greatly by the processes of fabrication required to transform the raw materials into the shapes in which they are used. It does not belong to the metallurgist to trace these materials through all the transmutations which they may undergo before being put to use. The very conditions of use often require special treatment, and do certainly enter largely into the inquiries which must be instituted to prove their fitness for special purposes. On the other hand the engineer has not usually the appliances nor the

opportunities, for fully testing the materials he employs. Time, money, and apparatus are necessary, neither of which can usually be spared from his ordinary professional engagements.

For the action of the ordinary materials used in this country reference can only be had to the results of researches by private individuals made on a limited scale, or to more general and systematic, but less applicable, results of experiments instituted by foreign governments.

It is not to be expected that any system of experiments, however carefully conducted, on the properties of the metals, will cover every special case, but there are standard products, and standard processes of fabrication, for which exact determinations of absolute strength, ductility, elastic limits, and other properties, under given conditions of use, may be ascertained.

The rapid changes which are taking place in the production of iron and steel, especially, render such investigations on a scale commensurate with the importance of their useful applications a matter of the highest interest.

It is becoming, in fact, a reproach to our country, that for such investigations, which, on account of the magnitude of the work to be accomplished, can only be undertaken at public expense, so little has been accomplished here. Our engineers are often obliged to refer to experiments made nearly half a century ago for the numerical data necessary for designing the most simple structures.

The names of Hodgkinson and Fairbairn will always be mentioned with veneration, and held in honor, as men whose experimental researches have for more than a quarter of a century formed the groundwork of our structures in iron. But even the youngest engineers become restless under the necessity of constant reference to results which they recognize as belonging to the past and as not wholly applicable to the present. Much useful knowledge has been, from time to time, contributed by our own engineers on this subject, but with some exceptions their results have been generally limited in their range of applications, and not sufficiently comprehensive to become of universal use, on account of want of time and means for prosecuting extended investigations.

It is often argued that it is essential for an engineer to make special tests of the materials he employs in the construction of all important works. While this is undeniably true to a certain

extent, it is also true that an engineer cannot *design* a structure without *assuming* certain data, among which, and the most important of which, are the properties of the materials he proposes to use.

Truly scientific construction demands that all the parts of a structure shall be in harmony, in weight and strength, with the external forces which act upon them, under the conditions of use to which they are subject; whether such a structure be a machine, a bridge, or a public building, the designer must base his construction on these truths, and the want of correct data in any element of the design must entail either loss or damage.

The influence of experimental research in building up the science of pure, or abstract, mechanics shows the impossibility of progress without direct reference of probable laws to the test of experiment.

The dynamic theory of heat remained a speculative question until a few simple and decisive experiments settled all controversies, and opened the way for Rankine and Clausius to develop the new and before unknown science of Thermo-dynamics. We find La Place enunciating this theory in specific terms, and in language identical with that now employed, when merely stating the opposite views of heat entertained in his day; the great mathematician himself upholding until his death the material theory — a theory which is now mentioned only to illustrate, what seems to us in the light of later developments, almost unaccountable ignorance on the part of some of the most learned scientists of the past age. It is true that years of controversy and discussion finally prepared the minds of scientific men for the acceptance of the dynamic theory; but, nevertheless, it required the experiments of Joule to furnish the proof of the theory by showing the absolute identity of heat phenomena, of the most simple kind, with those of ordinary mechanics. And when, finally, through the experiments of Regnault on the specific heats of gases, means were afforded for an independent determination of the mechanical equivalent of a unit of heat, controversy and discussion ceased. These two independent determinations, one by direct experiments and the other through analysis employing the specific heat of air as finally determined by Regnault, were indisputably, when taken together, and especially in view of the importance of the results, the most interesting and important instances of the value of experiments in establishing exact theories in the whole history of science.

The student of mechanics of to-day may not see before him the prospect of brilliant discoveries of new principles, but by patient and faithful labor in the field of experimental investigation, there are still opportunities of adding to mechanical science a knowledge of truths which may have a lasting and beneficial influence in promoting the applications of this science to the Arts and Industries.

I need but refer in this connection to that other series of experiments of Regnault on the elastic force and temperature of vapors, as one of the most signal instances of the value of experimental research, when properly conducted, which can be mentioned. Although by these experiments nothing was added to establish the theory of heat, yet in connection with the universal use of steam as a motive power, and for other purposes, the results have been of vast importance. For accuracy and completeness, these investigations stand unrivalled. They form the basis of all the important applications of the theory of heat to vapors, and thus constitute the foundation for all calculations relating to steam in its various uses. They became not only of immediate use to the practical engineer, but they enabled Rankine, Clausius, and Zeuner to extend the dynamic theory to the study of vapors, and thus to cover all ordinary technical grounds.

Of all the subjects in which experimental research is now demanded, there is none more important than that to which allusion has been already made: the strength and properties of materials used in constructions. It is needless to specify the various kinds of investigation demanded, for our knowledge of the subject, as far as the materials produced in this country are concerned, is in a most uncertain and unsatisfactory state. Boldness of design, and a necessary increase in magnitude of the various parts of structures, are the inevitable tendency of engineering progress. Even in commercial buildings, in which iron scarcely entered as an important element twenty years ago, single cast iron columns have recently been introduced which are required to sustain, permanently, nearly one thousand tons. Similar instances of the use of wrought iron and steel in tension and compression might be given in which for the large cross-sections employed no reliable data for strength is available.

A most important question to be considered where experiments involving large expenditures, expensive apparatus, and much time,

are to be made, is the constitution of the body or commission having charge of such investigations. It is doubtful whether any body of scientific and practical men, however competent they may be, can conduct and control properly the details of such important and laborious work, if they are at the same time burthened by the cares and demands of other private or professional duties. Success in scientific work of this character depends largely on each specific investigation being committed to one person employed solely for the purpose, and whose acquirements and enthusiasm for his subject, need the least amount of advisory aid.

A commission of experts may well direct the character and aim of special investigations, and to some extent the methods to be employed; but without the stimulus of personal responsibility, combined with ardent zeal and special aptitude, of some one who has entire control of details, and who through his constant devotion to one object may reap the principal rewards of his own discoveries, experimental investigations of this kind are apt to end in failure.

Other subjects on which further researches are desirable will readily present themselves. Among these may be mentioned, as of much practical interest, the flow of air and steam through pipes and conduits.

The transmission of power by compressed air, and the adaptation of apparatus for ventilating and heating public and private buildings, require a knowledge of the resistances and other phenomena connected with the flow of air and steam, the available data for which are meagre, and rest upon very incomplete experimental foundations. The complications of this subject, arising from the changes of temperature of the flowing air or steam, and the difficulties thereby caused in attempts to measure quantities of the flowing liquid, render experiments in this direction so tedious and expensive as to operate almost as a bar to ordinary individual efforts.

The importance of the questions involved, especially in connection with sanitary appliances, is, however, becoming greater each year. In this connection also the laws of the transfer of heat, and their technical applications, assume new interest. In many of our large cities works have been erected or are in progress, involving great expense, having for their object the supply of steam for heating and ventilating purposes, and for power, by transmitting

the steam in underground pipes throughout large districts from a few central stations.

These undertakings present numerous problems which have not heretofore been encountered in engineering operations. Correct data concerning the laws of the flow of gases and vapors, and the transfer of heat, constitute an important element in many of these problems. While compared with similar data for the flow of liquids, it can hardly be said that the knowledge available to the engineer is reliable or satisfactory.

In the construction of public buildings, private dwellings, and factories, the necessity of including in the first design complete arrangements for warming and ventilation, has of late years been forced upon architects and engineers by a public demand for healthful arrangements; and all the scientific and technical knowledge that can be brought to bear on the subject is desirable. In this direction but little has been accomplished in this country, except in connection with private practice. For information that cannot be obtained from this source, recourse must be had to foreign works, some of which, notably those of Peclét and Morin, may be taken as examples of the great value of such experiments as were made by these distinguished experimenters and authors.

It would be impracticable to present in detail all the principal subjects of practical mechanics in which more exact knowledge is needed in technical applications. There is a universal activity at the present time, everywhere, towards overcoming obstacles to what is called progress.

The demand is constantly for greater achievements. The public calls imperiously for greater speed on railways and steamships, and the engineer must meet the demand. Greater speed requires heavier rails, heavier engines, larger ships, and stronger machinery. But the same imperious public also insists on absolute safety, and reasonable cost. A train must fly at high speed across long bridges, built at the least possible expense, but woe to the engineer if a calamity occurs. No explanations are received; the public will accept no part of the blame.

This increasing ambition for something faster, grander, more convenient, or more healthful, brings with it increasing risks and responsibilities to professional men. The members of the engineering profession, especially, have to assume great risks, and it is not unreasonable that they should ask public aid, as our engi-

neering societies have done, to arrive at a kind of knowledge that can be procured in no other way than through public aid and expense, and without which their best efforts may often result disastrously. If we include in these general propositions other branches of technical science, especially Chemistry and Physics, it would not be hazarding too much to say that the continuance and permanence of what is usually called modern progress depend greatly on a general recognition of the importance of this kind of scientific research.

There is one field of research which alone may well occupy the attention in this respect of the best minds of the age, and which ought to receive the most generous public support. That field is sanitary science, or sanitary engineering. Wordy controversies can never satisfactorily settle questions of ventilation, which involve force and motion, the heating of air and its conduction and distribution; nor questions of poisonous emanations or solutions, in air or water, nor questions of house and street drainage. Connected with these subjects there are abundant opportunities for systematic experiments, not only in the domain of physics and chemistry but of mechanics.

We cannot expect to rival the nations of antiquity in architectural achievements, although it would seem that lavish expenditures on monumental buildings most readily satisfy public ambition even in this age; but it is to be hoped that the generous impulses which have begun to manifest themselves in the direction of sanitary improvements may absorb a full measure of this ambition and direct it towards the most perfect provisions for the health of communities, so that social life for the poor as well as for the rich may rest on the highest possible plane of physical as well as mental and moral excellence. A small share of the millions expended by the people in the architectural masonry and decorations of public buildings, would, if properly devoted to mechanical, physical, and chemical researches connected with sanitary engineering, accomplish much towards this end.

There is undoubtedly much force and wisdom in the suggestive remark of one of the most distinguished sanitary engineers of England, that "if he were permitted to build the homes of a people, he would not care who should write their songs, or make their laws."

There is a branch of mechanics which has from time to time

excited considerable public attention, but which, from its not presenting features of great public utility, has not been ranked among those having a commanding importance.

It is only necessary to mention *animal mechanics* to bring to mind the beautiful experiments of Marey in determining the movements of animals in the ordinary processes of locomotion. A study like that of Marey of the mere motions of the locomotive appendages of animals — the curves described by the extremities in the operations of flying, walking, and swimming — must always remain barren of useful results unless connected with the dynamical features of the problems involved. These Dr. Haughton, of Trinity College, Dublin, has supplied, by a most interesting treatment of the subject, and by careful experiments, extending over a period of ten years.

Muscular force is so related to life that it appears almost to furnish the only unmistakable evidence of vitality. When the involuntary and voluntary muscles of the animal system cease to act, life can no longer be said to exist. Dr. Haughton has discussed the whole subject of muscular forces not only in their relations to the skeletons through the medium of which they act, but in their properties of strength, endurance, refreshment, and fatigue, in such a manner as to throw new light on this interesting subject, and to suggest its real useful aspects in connection with the laws of health and physical development.

The conformity of the phenomena observed and the principles established, in regard to movements connected with animal life, with the laws of ordinary mechanics, is complete. The four elementary machines, which constitute either singly or in various combinations the bases of all artificial machinery, are not only found to be represented in the various kinds of animal locomotion, but, except perhaps in some of the very lowest orders of animals, no other means of locomotion can be discovered.

In relation to the expenditure of energy, it appears to have been demonstrated also, that the sources of muscular, and probably of nervous energy, are, as in the ordinary expenditures of force, subject to exhaustion at rates which increase in a high degree with the rapidity with which work is performed.

The space which a steamship can traverse, with a given amount of coal for consumption, varies inversely as the square of the speed; and the number of days required for the exhaustion of the fuel,

inversely as the cube of the speed. Similar laws are applicable, as Dr. Haughton has shown, to human exertions and furnish scientific and mechanical reasons, why the prolonged, intense nervous activity of business life, so much complained of in these days, results in premature and permanent disabilities. Doctor Haughton was the first to demonstrate experimentally the laws of fatigue and refreshment of muscular action, and to suggest, at least by very natural inferences, that there are limits of exertion within which alone these processes may be expected healthfully to balance each other.

In one sense all great works, successfully executed, furnish data and precedents for the future. Each one reaches out beyond its predecessor, the result being the gradual building up of an art, an industry, or perhaps a profession.

In exceptional cases a single construction, or a single exploit, revolutionizes practice. Two episodes of our late war present signal examples in this respect. The conflict between the Monitor and Merrimack in Hampton Roads furnishes the first real experimental test of the invulnerability of iron-clad vessels, and the efficiency of the turret system devised by Ericsson; while the demolition of Fort Pulaski by General Gilmore, by batteries placed at one and two miles distance, destroyed, in a day, all faith in defensive works of masonry, which up to that time had been regarded as the bulwarks of seacoast defence. History furnishes no instance of so complete and universal a change in practice caused by one gigantic experiment occupying hardly more than twenty-four hours. The echoes of Gilmore's guns had scarcely died away before orders were issued from the war offices of all nations having seacoasts to defend to suspend work on masonry fortifications.

These two events, the conflict of the Monitor and Merrimack, and the reduction of Fort Pulaski, were the beginnings, however, of a contest which has lasted to the present time — the contest between rifled ordnance and defensive armor. In this war of force, of impact and resistance, if we take into account the cost of armored vessels, hundreds of millions of dollars have been expended.

In the course of ordinary events, however, isolated results do not furnish the most useful collection of facts; there are few branches of art or industry, involving mechanical appliances,

which may not be advanced by investigations systematically undertaken, and which are made through a careful observance of all phenomena and conditions to bring out general laws.

It will not be considered inappropriate in connection with this subject to make a brief allusion, and render tribute to the works of one who, though not now living, may be regarded as having been among the foremost investigators of the present age in the department of Mechanical Science. No name is more frequently mentioned, wherever the English language is spoken, as authority in matters relating to applied mechanics than the name of RANKINE. His original investigations in founding, and building up, the science of thermo-dynamics, were sufficient to establish his fame; but the peculiar fascination of his works, to the engineer, lies in the wonderful scope of his genius in the solution of problems of applied mechanics in every branch of engineering, and the remarkable skill and perception with which he introduced experimental data into his researches; thus giving to his deductions the most unerring scientific exactness combined with the nearest approximate practical solutions.

Unlike the writings of many scientific authors, Rankine's voluminous works will not bear reduction or condensation. Conciseness of style and the shortest modes of demonstration are their marked characteristics. Every paragraph, and almost every line, seems to contain the expression of an idea, a valuable description, or the enunciation of a principle.

As an investigator he appears never to have touched a subject without throwing new light upon it, either by his mode of treatment, the discovery of new principles, or in extending its practical applications. Although not an experimenter himself, no investigator has made more constant use of well established results of experiments made by others, or has been more skilful in bringing useful rules of practice out of the combinations of such results with exact theories. He has left to the present generation the most complete exposition of the principles of the science of applied mechanics that can be found in the English language.

England has not yet done full justice to the memory of one whose labors in science ought to be regarded as having added to her national fame, and whose practical expositions have contributed so much to the progress of all nations in the Mechanic Arts.

PAPERS READ.

On a Newly Discovered Absolute Limit to Economical Expansion in the Steam Engine and in other Heat-motors.

By Robert H. Thurston, of Hoboken, N. J.

[ABSTRACT.[1]]

It has been universally assumed by engineers, and probably by all physicists familiar with the theory of the steam engine hitherto accepted, that the best ratio of expansion, whether for maximum efficiency of fluid, of engine, of capital, or of plant,[2] increases with increase of steam pressure without limit, and that such ratio may be indefinitely increased with decrease of the ratio of back-pressure for any one kind of engine, notwithstanding the fact that the value of the ratio of expansion is modified by variation of the conditions of working, even where the ratio $\frac{p_b}{b_1}$ is the same. The writer proposes to show that, in every actual engine, in every engine operated under the conditions of real work and of usual practice, there exists a limiting value of any of these "ratios of maximum efficiency" beyond which it cannot be economically raised even with an infinitely elevated boiler-pressure. It will be further shown that this "absolute limit" may be readily, and probably often is, passed in everyday practice, that in the usual forms of steam engine an absolute limit exists within or not far beyond the customary working range of expansion, beyond which expansion cannot be carried with economy, however high steam pressure may be adopted; in other words, with infinite pressure, the economical value of the ratio of expansion will be found often not merely finite, but sometimes probably within the limits of familiar practice.

In earlier papers[3] the writer has shown that the conditions

[1] This paper is published in full in the Journal of the Franklin Institute, Sept., 1882.

[2] *Vide* a paper "On the Several Efficiencies of the Steam Engine," etc., Journal of the Franklin Institute, May and June, 1882; *Transactions of the American Society of Mechanical Engineers*, 1882.

[3] Journal of the Franklin Institute, May, 1881, February, May, and June, 1882; *Transactions of the American Society of Mechanical Engineers*, 1881-2.

under which engines are worked in actual practice differ in a very important degree from those assumed in the simple thermo-dynamic theory of heat engines, and, especially, that the unjacketed steam cylinder, which is usually assumed to be of non-conducting material, is to such a serious extent a reservoir and a transferrer of waste heat that the efficiency of the engine is greatly and sometimes enormously reduced.

The equations which would give values of the ratios of maximum efficiency for the ideal "perfect engine," therefore, do not apply to real engines," and the author has deduced[4] modified equations, which are at least approximately correct for real practice. He has also produced true "curves of efficiency" from real engines, which were found to be practically the same in form and location, to be practically identical in character, with those inductively obtained, so far as the two could be compared.

"THE GENERAL EQUATION OF ALL STEAM ENGINE EFFICIENCIES," is

$$\frac{1}{E} = \frac{A v_2 + B v_2 r^{-1}}{2R\left(b p_1 v_2 \frac{n r^{-1} - r^{-n}}{n-1} r^q - p_b v_2\right)} \qquad (A)$$

which becomes a minimum, and makes the *commercial efficiency* of an engine doing the required work a maximum when, to obtain r, we have made

$$r^q + \frac{q}{M(q-1)} r^{q-1} - \frac{q-n}{n(q-1)} r^{q-n+1} \frac{q-n+1}{Mn(q-1)} r^{q-n} = \frac{n-1}{Mnb(q-1)} \frac{p^b}{p_1} \qquad (B)$$

The ratio $\frac{p_b}{p_1}$ is the quotient of the total back pressure by initial pressure, all useless resistance being included in p_b. A is the measure of annual costs, variable with size of engine, B of costs variable with quantity of steam demanded, v_2 the volume of steam-cylinder, r the ratio of expansion, R the revolutions per annum, b the proportion of steam in the cylinder when $r = 1$, p_1 is initial steam pressure, n is the exponent of the familiar equation of the steam line, $pv^n = \text{const.}$, and r^q is the rate of variation of losses with increase of r. $M = \frac{A}{B}$.

When instead of M, N is inserted in the equation, N being the quotient of *all* annual expenses, independent of fuel supply, by

[4] "On the Several Efficiencies of the Steam Engines," etc.

all such expenses variable with fuel and steam supply — the expression determines the ratio of expansion at "maximum efficiency of capital invested in a *given plant*."

When M or N is made zero, the expression reduces to:

$$\left(r^{q-1} - \frac{q+1-n}{q\,n}\, r^{q-n}\right) \div \frac{n-1}{b\,q\,n} = \frac{p_b}{p_1} \qquad (C)$$

and gives the ratio of expansion at which maximum "duty" or "efficiency of engine" is attained.

When $b = 1$ and $q = 0$, the case becomes that of the ideal engine working the fluid in a non-conducting cylinder, and these equations become, for efficiency of capital and of engine,

$$r^{-n} - \left\{\begin{matrix} M \\ \text{or} \\ N \end{matrix}\right\} \frac{n - n\,r^{1-n}}{n-1} = \frac{p_b}{p_1} \qquad (D)$$

$$r^{-n} = \frac{p_b}{p_1} \qquad (E)$$

Studying these equations, it will be seen that, in all except the last (E), it is possible to find finite values of r such that their left-hand members shall reduce to zero; as in them n is nearly always equal to unity; q varies from $q = 0$ to $q = 0.3$ in good practice, and b usually ranges between $b = 0.8$ and $b = 0.9$; M or N is usually between 0.02 and 0.15, and the form of the function is such that the first member may always be made to disappear for some finite value of r. Then we shall have in (B)

$$r^q + \frac{q\,r^{q-1}}{M(q-1)} - \frac{(q-n)r^{q-n+1}}{n(q-1)} - \frac{q-n+1)}{M\,n\,(q-1)} = o; \qquad (F)$$

and in (C)

$$r^{-n} - M\,\frac{n - n\,r^{1-n}}{n-1} = o$$

$\frac{p_b}{p_1} = o$; $\frac{p_1}{p_b} = \infty$ and the value of r, at which this condition is obtained, constitutes an "absolute limit," for the case taken, beyond which expansion cannot be carried economically, even with steam increased to infinite tension; beyond this point $\frac{p_b}{p_1}$ becomes negative, indicating the assumption of impossible conditions.

Examining equation (E) we find no such limit; the sign of the first member remains positive for all values of r, and can never become zero for a finite value of that quantity. We are thus taught that an important difference exists between the *ideal* engine, with its non-conducting cylinder, and the *real* engine working steam in

a metallic cylinder, as well as between the case of maximum efficiency of engine and that of maximum efficiency of capital. In the cases of maximum efficiency of fluid and of engine for the ideal perfect engine, equation (E), only, is it true that indefinite increase of steam pressure permits indefinitely increased expansion. In all other cases an absolute limit exists, fixed for each case, beyond which expansion cannot be economically carried.

The above equations for real engines are approximate, and are practically exact — the values of the constants being determined with accuracy — within the range met with in practice, and the conclusions here deduced are, therefore, correct, although the exact values of the constants, or even the precise form of the variable function, r^q, here may not be fully ascertained.

Should it seem desirable, it is easy to deduce the same conclusions — and thus to confirm, by independent proof, the above proposition — by the examination of the "Curves of Efficiency" of ideal and of real engines.[5]

Thus it is seen that, while the ratio has no limit for the ideal case, it has such a limit for the real engine, and that this limit may be found at a low ratio of expansion. The writer has made this comparison for the U. S. steamers "Michigan," "Georgiana," and "Bache," for which three cases the real curves have been obtained by him,[6] and finds that, these curves remaining unchanged,[7] it is impossible economically to increase the ratio of expansion in such engines beyond three, five, and ten respectively, even with unlimited steam pressure; *i. e.*, even when $\frac{p_b}{p_1} = 0$.[8]

We may therefore conclude:

(1) That in all engines there exists an "absolute limit to the economical expansion of steam," whether considered with reference to efficiency of fluid, of engine, or of capital, which limit can not be passed, whatever pressure of steam may be carried up to the point of cut-off.

[5] With such curves the ordinates measure the work done at various ratios of expansion in any given engine, by quantities of steam proportional to the abscissas.

[6] *Vide Transactions of the American Society of Mechanical Engineers*, 1882; JOURNAL OF THE FRANKLIN INSTITUTE, June, 1882.

[7] The form and location of these curves at very high pressure would undoubtedly be somewhat altered, but it is evident that there must still be found this absolute limit so long as condensation increases with extended expansion. The writer is inclined to believe that the form of the curves will be substantially as shown, however, at any attainable pressure.

[8] The "Michigan" has an unjacketed condensing engine, using saturated steam; the "Georgiana" a similar class of engine, with superheated steam; the "Bache's" engine is jacketed and compound.

(2) That this limit is found at higher ratios of expansion as the type of engine is more efficient, but that the limit is indefinitely removed only in the ideal engine, and then only as affecting the ratios of expansion at maximum efficiency of fluid and engine.

(3) That this limit is found at a small value of the ratio of expansion in ordinary engines, and therefore may be readily passed in every-day practice.

The limit is not far from three in the common, unjacketed condensing engine, four or five in the same engine using superheated steam, and ten or twelve in the ordinary compound engine.

(4) It is evident that the general propositions of this paper are true of all heat engines having fluid working substances, whether vapors or gases worked in metallic cylinders.

Transverse Strength of large Spruce Beams. By Gaetano Lanza, of Boston, Mass.

[ABSTRACT.]

The moduli of rupture and the moduli of elasticity of timber, given in our text-books and engineers' hand-books for practical use in construction, the former for determining the breaking weight of beams, and the latter for determining their deflections under given loads have, in almost all cases, been deduced from experiments on small beams, not at all comparable in size with those in common use in building.

While such a course is, to-day, recognized as misleading by our best engineers, it has been and is still followed very extensively, because, with a very few exceptions, we have had no experiments on the strength or deflection of full size wooden beams subjected to a transverse load.

Again, no experiments, as far as I know, have been made on the large scale on the strength of such timber joints as are used in framing, where mortises and tenons are employed, or where notches are cut, and we have had no experimental basis from which to deduce their strength, so that the loads which have been supposed or assumed to be their breaking loads are probably very far from the true breaking loads.

In view of these facts, and of the practical importance of experiments on full size pieces to the engineer, the architect, and the

builder, I have been so desirous of having, at the Massachusetts Institute of Technology the means of carrying them on, and of giving my students the opportunity of performing them, that I have had constructed a machine capable of testing a beam transversely loaded with a load not greater than 50,000 pounds, provided the beam has a length not exceeding twenty-five feet.

By its means we are also able to test most of the framing joints used in practice. The results actually obtained by testing spruce beams are given in the following table:—

Number of test.	Width and depth in inches.	Distance between supports.	Manner of fixing the ends and loading.	Breaking weight in pounds.	Modulus of rupture in lbs. per square inch.	Trautwine's form. Central breaking weight of beam 1" square and 1' long supported at ends.	Barlow's form. End breaking weight of beam 1" square and 1" long, fixed at one end only.
1	2 x12	6' 8 "	Framed at ends.	6,304	.....	.....	.....
2	4 x12	6' 8 "	Header.	10,338	.....	.....	.....
3	2 x12	15'	Load at middle.	5,794	5,432	302	906
4	2 x 9	6' 7.5"	" "	7,227	5,320	295	887
5	2 x12	15'	" "	5,586	5,237	291	873
5 (a)	2 x12	7'	" "	8,982	.....	.....	.....
6	2.75x 9	6' 8 "	" "	7,586	4,082	226	680
7	3 x 9	4'	" "	11,086	3,285	183	548
8	3 x 9	10'	" "	6,086	4,508	250	751
9	3 x 9	15'	" "	5,086	5,651	314	942
10	3.88x12	20'	" "	6,586	4,253	237	709
11	2.5 x13.5	10'	" "	9,585	3,787	210	631
12	3.75x12	16'	Load 4.5' from one support.	7,585	3,271	182	645
13	3.75x12	16'	Ditto mortised.	9,085	.....	.....	.....
14	7 x 2	7'	Load at middle.	1,944	8,748	486	1,458
15	1.75x 6.75	7'	" "	4,785	7,562	420	1,260
16	3 x 9	6' 8 "	" "	9,985	4,931	274	822
17	3 x 9	6' 8 "	Load distrib. at 4 points, 16" apart.	16,744	4,961	276	827
18	3.9 x12	16'	Load 4.5' from one support.	12,585	5,218	289	869
				13)59.936			
				4,610		256	768

As to the quality of the timber, it was, with the exception of Nos. 14 and 15, all common merchantable stock, its average modulus of rupture being 4623 pounds per square inch, the largest being 5651.

Nos. 14 and 15 were cut in 1877 and had been seasoning on the wharf since that time till they were tested in April, 1882.

Nos. 1, 2 and 13 are tests of framing joints, and hence from them we cannot determine a modulus of rupture. No. 5 (*a*) was a piece sawed from one end of No. 5, after this had been broken; hence, from this no modulus has been deduced.

The question must now be answered how these moduli compare with those commonly given in our text-books and engineers' handbooks.

	Max.	Min.	Mean.
Hatfield gives	12,996	7,506	9,900
Rankine gives	12,300	9,900	11,100
Laslett gives	9,707	7,506	9,045
Trautwine gives			8,100
Rodman gives			6,168
These tests have given for merchantable stock	5,651	3,271	4,623

Hatfield's, Laslett's, Trautwine's and Rodman's figures are from their own experiments, and, as is evident from the above table, they differ very considerably. Trautwine advises for practical use to deduct one-third, on account of knots and defects, hence after making this allowance he would use 5400. My own figures would give this only for the best of the stock in common use, and in the case of knotty and poor timbers would give much lower values; I obtained as low as 3271 and an average of 4623.

It will be observed that although Rodman's result is not at all as small as mine, it is smaller than any of the others; this is to be accounted for by the fact that the pieces which he experimented on were $5\frac{3}{4}$ inches deep, $2\frac{7}{8}$ inches wide, and 50 inches between the supports, being larger than those generally employed by other experimenters, though not at all as large as those used in practice.

Only three tests have been made to determine the modulus of elasticity, and these have given respectively as follows:—

No. 3	1,237,215
No. 4	1,067,893
No. 6	938,453
	3) 3,243,561
Average	1,081,187

The machine has only been at work about two months, and hence no more work has yet been done, but these and other series of tests will be made in the future, and it is hoped will furnish us some more information of practical importance.

NOTE.—Since the above paper was delivered a much larger number of tests have been made, and a great deal more information obtained on the subject, a part of which was published in the Franklin Institute Journal for February, 1883.

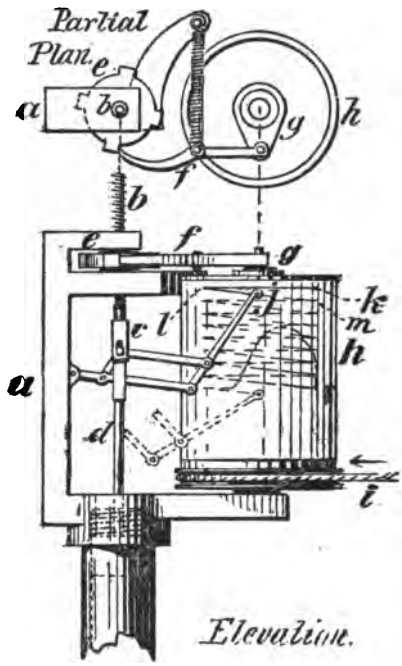
A METHOD OF CUTTING SCREWS OF INCREASING PITCH. By J. BURKITT WEBB, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

A SCREW whose pitch increases uniformly from one end to the other may be cut by the following method.

Mount a suitable cutter, say one formed like a short reamer, with teeth also on the end surface (a "Brown and Sharpe" cutter for the teeth of wheels could also be used), in a milling machine, and on the bed of the same fasten two journals, so arranged that the blank for the screw may either revolve or slide in them without shaking. The screw-blank as it rests in these journals must, of course, be in the proper relation to the cutter, so that if it were moved in the direction of its axis, a groove parallel thereto would be milled in the blank, and if it were revolved a groove would be milled around the blank, while by a suitable combination of these

motions any desirable screw would be milled. Suppose now that by some means, we get, say, a quarter of a turn of a screw of any form milled, and that we then attach to the bed of the machine a pin, of the same size as the cutter and somewhat in advance of it, to act as a nut. The end of this pin must fit in the groove in exactly the same way as the cutter itself, so that by turning the screw-blank it will feed past this pin as if it were a small portion of a nut. It will now be seen that by arranging a feed to revolve the



screw-blank we can make the latter screw forward past the fixed pin, and that, therefore, the cutter will extend the groove indefinitely. It is also clear, that the form of the original portion of the groove (the form of the section of the groove is not the form meant) will be repeated throughout the screw.

In order now to make a screw whose pitch shall increase in arithmetical progression, we have only to make the original portion in accordance with this law, and then to arrange a second feed (the ordinary feed of the milling machine) by which the bed with

all that is fast to it may be uniformly moved a certain distance in the direction of the axis of the screw-blank for every revolution of the latter.

By reversing this latter feed, on passing the middle of the blank, a screw-cam can be cut, which shall give an oscillation of its nut, and by suitable variations of this feed many other forms can be cut.

Indicator Attachment for High Speeds.

By J. Burkitt Webb, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

The steam-engine indicator, in the form given it by Watt, is essentially a low-speed instrument, and momentum and friction will ever be present to vitiate its indications for the high speeds coming into use. A new principle must, therefore, be adopted, which shall make the action of the instrument independent of the speed of the engine.

This new principle has been embodied in an instrument quite different from the ordinary indicator, and also in an attachment to be applied to the latter; as the former contains, besides the essential and radical improvements, several conveniences, the shorter and simpler description of the latter will be given.

The new principle may be thus explained:—in the ordinary instrument we have a positive, horizontal, oscillating motion of the paper, coinciding with that of the piston, and a non-positive, or free, vertical motion of the pencil dependent on the steam pressure in the cylinder; in the new form of instrument the first motion is the same, but it is made more certainly positive; for the second motion, however, is substituted a positive vertical-feed of the pencil, dependent on the first motion. These two motions form on the paper what we have called the "blank-diagram," which will, from the manner of its formation, and in the figure, be seen to consist of a parallelogram of zigzag "pressure lines," k, l, m, etc., such as would be made by oscillating (instead of continuously revolving) a chronograph cylinder. This arrangement evidently eliminates

the troublesome effects of momentum, and the blank-diagram is perfectly accurate.

The volume and pressure curve is formed upon this blank-diagram by a third or "registering motion," which introduces what we may call a "chronograph" jog in the pressure-lines as they cross this curve. This motion is dependent, of course, on the steam-pressure, and being exceedingly small it allows of the employment of a diaphragm, instead of the ordinary piston and cylinder, thus eliminating the injurious effects of friction and making it possible to get rid of the cock and of most of the waste room between the indicator and the steam-engine cylinder.

We think it no disadvantage that this new form of instrument requires a number of strokes of the engine for the completion of a diagram; in fact, in the ordinary instrument no attempt is made to confine its action to a single stroke.

Experiments to Determine the Strength of Cylinders with Domes Attached. By Samuel Marsden, of Saint Louis, Mo.

[ABSTRACT.]

These cylinders were made in imitation of a locomotive boiler with a dome, and the object of the experiments was to ascertain one of the ways in which such a boiler gives way under pressure. Five cylinders were tested, they were about four inches in diameter, and their domes varied in size. They were of lead and the domes were soldered to them; this material was chosen on account of its uniformity. The holes through the walls of the cylinders, which connected their interiors with those of the domes were concentric with the latter, but not in all cases of the same diameter, and this proved to be the point of main importance. When this hole was made as large as the dome the cylinder and its dome burst by cracking in a plane passing through the axis of both, the crack starting on both sides of the dome, at the angle between dome and cylinder; but when this hole was made small the cylinder burst on the side opposite to the dome, much the same as an ordinary cylinder would have done.

AËRIAL NAVIGATION. By JOS. DE L'ETOILE, of Ottawa, Canada

[ABSTRACT.]

THIS paper was a review of the subjects of atmospheric currents, electricity and gases with a view to practical aërial navigation by means of balloons: to take from the air the means to navigate through the air, and thus do away altogether with the ballast and all such dead weight material which has been and still is the real cause of the little or no progress at all, having been made with regard to the guidance of balloons. The balloon should take the form of a fish and be provided with a propeller, a rudder, an air compartment, gas and air pumps or exhausters, electric battery, electric meter, and two safety valves. The balloon should be made to rise slowly and to descend with ease to the earth. All danger from electric flashes coming in contact with the escaping gas through the envelope of the balloon has been obviated by using material which prevents such escape. The above plan was tried, in the year 1869, and it worked satisfactorily. When the ensemble of this new aërial vessel shall be put to a test, it will, no doubt, prove as important to the aëronaut as the steamboat and locomotive are to the engineer.

Atmospheric currents vary at different altitudes, and by ascending through high atmospheric regions, aëronauts cannot fail to meet with favorable winds. Observations for scientific purposes, taken during ascensions in balloons, have hitherto been but imperfectly made. Globated balloons usually rise in an oblique direction and under the combined influence of the direction of the wind. The change of temperature in the atmosphere in a vertical ascension could not naturally be the same as that met with in an oblique direction and sometimes at twenty or fifty miles horizontally from the starting point, as the atmosphere above New York could not be the same as that met with above Boston at the same altitudes, at the same hour and minute, and under the same influence of aërial current. For want of practical steerable balloons, the regions of the two poles of our globe are yet unknown to geographers. The different atmospheric currents at certain altitudes, and at different places in a given hour and minute, are still a puzzle to meteorologists. The capacity of telescope glasses

has not as yet been ascertained at different altitudes and under different atmospheric conditions.

Astronomers seem to have overlooked the fact that a glass cannot be as true to the eye when looked through on the surface of the earth, as it would if above atmospheric electric currents. European and American governments have, without forethought, sent brave men, on board of ships which cost millions of dollars, to the arctic regions, and what has been the result? Of course, great loss of lives and the sinking in the deep of all such valuable property. Let any one beg government aid for the minimum sum of say, four thousand dollars, which would be sufficient to meet the expenses of building a steerable aërial ship; what would probably be the answer?

The Future of the Balloon as a Practical Means of Aërial Travel. By W. H. Lynch, of Danville, P. Q., Canada.

[ABSTRACT.]

The two systems of aërial navigation, or rather the system of actual aërial travel by balloon and the theory of aërial navigation by utilizing the flying principle, are quite opposite.

In the flying machine the propeller or wings are required to act upon the air at once to lift the dead weight of the machine, and to propel it or direct its course. The balloon, on the contrary, is itself a lifting power, lifting itself and carrying foreign weight that may be a force to direct or propel it.

We do not forget that the self-lifting machine is in one respect at a disadvantage with the inert or dead-weight machine. To be lighter than air means to be bulky, and to offer in motion greater resistance to the air, while what is heavier than air is more compact, and its motion more easily caused or directed. This one advantage of the flying-machine over the balloon counts, however, but little so long as it cannot both lift itself and carry the little extra weight of the force required to propel it. The balloon will carry no mean weight besides its own. It will carry, besides possible passenger or freight weight, the extra weight of a force that may direct or even propel the whole.

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